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# DIARRHŒA AND CHOLERA:

THEIR

NATURE, ORIGIN, AND TREATMENT

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BY

JOHN CHAPMAN, M.D.

M.R.C.P., M.R.C.S.

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1866

TO

PROFESSOR CLAUDE BERNARD,

MEMBER OF THE INSTITUTE OF FRANCE (ACADEMY OF SCIENCES), AND OF  
THE IMPERIAL ACADEMY OF MEDICINE;

PROFESSOR OF MEDICINE AT THE COLLEGE OF FRANCE, AND OF GENERAL  
PHYSIOLOGY AT THE FACULTY OF SCIENCES;

MEMBER OF THE ROYAL SOCIETY OF LONDON, OF THE ACADEMY OF SCIENCES  
OF ST. PETERSBURG, AND OF THE ACADEMY OF SCIENCES OF BERLIN.

DEAR SIR,

This Book owes its existence chiefly to your discoveries of the exact nature of the influence excited by the nervous over the arterial and the glandular system. If, guided by the light of those discoveries, I have succeeded in ascertaining the hitherto unknown nature of the most mysterious and appallingly resistless destroyer of man.— Cholera, and in pointing out the means of battling with it successfully, there is no one to whom I can so fittingly dedicate this account of my researches as to yourself. Be pleased, therefore, to accept this tribute as at once an homage to your genius, and as a grateful acknowledgment of the unspeakably important guidance which your contributions to Neuro-Physiology have afforded me. My chief aim has been to develop and reveal the practical truths which lay hidden in them, and to render those truths available for the alleviation of human suffering, and the saving of human life.

I am, Dear Sir,

Yours faithfully,

JOHN CHAPMAN.



## P R E F A C E.

---

THIS work has been written during the last two months in great haste and under peculiar difficulties, and is consequently much more incomplete even than it otherwise might have been. Indeed, I am painfully sensible of its defects, but deemed it wiser to publish it as it is at once, rather than withhold it until I could make it a more complete exponent of my views on the important subject to which it is devoted. And, at all events, I have, I hope, succeeded in placing clearly before the reader those pathological doctrines concerning diarrhoea and cholera, which are distinctively characteristic of the work, and which, if true, must inaugurate a new and scientific treatment of those diseases.

Of the several chapters constituting this volume, that on the "Causes of Cholera," is the one which I feel to be the least satisfactory: adequate data do not yet exist for enabling the discussion to be satisfactorily conducted. Nevertheless, by showing how known causes operate, I have, I hope, pointed out the direction in which further investigation of causes, as yet unknown, or only partially known, will be likely to be most successful. Much is known of the causal relation of solar heat to cholera, but much remains to be discovered: whether in the majority of cases it conduces to the development of the disease by its direct action on the body, or indirectly by inducing a subtle atmospheric state, as yet inappreciable, which is the intermediate influence originating the malady, needs careful investigation. When malaria co-operates to produce cholera, it is obvious that in such cases the sun's heat is the ultimate cause; but what is the relation of solar heat to the generation of ozone is still, I believe, unknown. Observations seem to have established the certainty



of the coincidence during cholera times of great heat and a deficiency of ozone, and I have shown how an absence of ozone is likely to result in cholera; but what is the immediate cause of the excess or deficiency of ozone in the air has yet to be ascertained. The relation between solar heat and atmospheric, as well as telluric, electricity is better known: it is, I presume, probable that all modifications of this force ranging from slight disturbances to the most violent electric storms, originate in the sun. There seems, however, sufficient evidence to prove, that in many cases of enteric disorder, varying in intensity from mere diarrhoea to choleraic collapse, disturbance of atmospheric electricity is the immediate cause of the disease. Whether simultaneously storms of telluric electricity usually occur, and whether, if so, these exert any considerable influence in the organism needs investigation.

There may already exist, for aught I know, in astronomical observatories, or elsewhere, records of observations of electric phenomena, both atmospheric and telluric during cholera times, sufficient, at all events, to admit of the institution of a provisional parallel between the various degrees of intensity of the cholera epidemic and of electric disturbances in the different localities in which the disease has prevailed. The vast extent to which submarine telegraphy is now being developed, might, no doubt, be rendered subservient in a pre-eminent degree for the solution of this important problem. In section four of the chapter on the "Causes of Cholera," I have quoted the statement, that during an outbreak of cholera at St. Petersburg, the degree of variation of the magnetic needle was unusual, and that the attractive force of the magnet decreased as the epidemic increased, and increased as the epidemic decreased. Should this statement be confirmed, it is obvious that electricity and magnetism are regions needing the most elaborate exploration in order to discover how far, and in what way, they contribute to the genesis of epidemic cholera. That they do contribute in a notable degree, cannot be doubted by any one who considers the facts mentioned in this volume. The precise mode of their

operation needs investigating, not less carefully than its extent. Is the atmosphere of those areas where cholera is epidemic, negatively or positively electric, relatively to surrounding regions? Are the magnetic currents on the earth's surface, within those areas more, or less, intense than normal? Does excess, deficiency, calm, or storm, of these subtle but potent forces, coincide with, if not contribute to, the production of epidemic cholera? As within circumscribed areas various electric and thermal conditions of the air induce thunder, wind, and rain, together or separately, during different lengths of time, and in all degrees of intensity, so it is conceivable, indeed probable, that in as far as these agents are productive of cholera the limitations of the areas of manifestation of their baneful influence, and the variations in the intensity and in the localities of their activity, will, subject to the modifying force of local circumstances and vital conditions, be analogous to the phenomena of atmospheric disturbance. If so, this consideration affords some clue to the remarkable and seemingly bizarre development of cholera,—now in one place, now in another, now simultaneous in many, now leaving a locality entirely, and again revisiting it in full force.

I have said nothing in the chapter on the "Causes of Cholera" concerning the mysterious "Cholera mist;" for as yet its coincidence with outbreaks of epidemic cholera is all that can be affirmed, and even the invariableness of this coincidence has not been established. Assuming, however, the usual presence of this mist, the fact that it is stationary, and immovable by the wind, seems to indicate that it is an expression of a peculiar electric or magnetic state of the air, and perhaps of the surface of the earth, and if so confirms the hypothesis that changes in the condition of atmospheric, and it may be of telluric, electricity, are prolific causes of cholera. Whether the bodies of cholera patients are, relatively to the bodies of healthy persons, negatively or positively electric is a subject which also needs a thorough examination. It behoves us to institute an exhaustive investigation of all these important questions, and so to discriminate between those causes of cholera which are endemic and removable, and

those which are epidemic—of cosmical, atmospheric, or telluric origin, and, therefore, beyond human control, in order that, after destroying every removable cause of cholera, we may be prepared to meet calmly those outbreaks of the disease, the causes of which are above the power of man, by the only means available, viz., the best possible treatment.

The very interesting and important questions of the contagiousness and mode of diffusion of epidemic cholera I have been obliged to omit altogether from consideration. The admirable summary of facts presented and discussed in a thoroughly cautious and scientific spirit in the "Report on the Cause and Mode of Diffusion of Epidemic Cholera," drawn up for the Royal College of Physicians by Dr. William Baly, affords excellent, if not adequate, data for a full discussion of the questions,—Is the disease contagious? and, How is it diffused? In a subsequent edition of this work I hope to devote a chapter to each of these subjects, and to prove that cholera is not the product of an organic poison; that it does not "travel" from place to place, as is affirmed in almost every history of its manifestations; that it is not contagious; and that the costly and vexatious international regulations, often involving great suffering, by which governments attempt to resist invasions of the disease are no defence whatsoever against its attacks, whereas both it and other diseases are often generated by the enforcement of the futile, and therefore cruel, as well as unjustifiable laws of quarantine.

JOHN CHAPMAN.

LONDON: 25, SOMERSET STREET,  
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*October 2, 1866.*

# CONTENTS.

## INTRODUCTION.

	PAGE
SECTION I.—NEURO-PHYSIOLOGY :	
The phenomena of disease . . . . .	1
The essential nature of disease . . . . .	2
Therapeutical desideratum and discovery . . . . .	2
Functional relation of the sympathetic nerve to the blood-vessels	3
Effects of cold and heat, applied along the spine, on the circulation of the blood . . . . .	4
Effects of cold and heat, applied along the spine, on the secretion of sweat and mucus . . . . .	4
Relation of the nervous system to the sweat and mucous glands	6
Innervation of glands generally, from the cerebro-spinal system	8
Positive and negative motor nerves . . . . .	8
Mode of action of positive motor nerves on glands . . . . .	9
Innervation of the sweat glands . . . . .	10
"      "      mucous glands . . . . .	10
"      "      salivary glands . . . . .	10
"      "      lachrymal glands . . . . .	10
"      "      mammary glands . . . . .	11
"      "      liver . . . . .	11
"      "      pancreas . . . . .	11
"      "      stomach . . . . .	11
"      "      kidneys . . . . .	12
"      "      generative glands . . . . .	12
Relation of the nervous system to every constituent element of the body . . . . .	13
Essential condition of healthy life . . . . .	14
Differences of constitution resulting from differences in the relative strength of the positive and negative motor nerves . . . . .	14
Summary . . . . .	14
SECTION II.—NEURO-THERAPEUTICS :	
Anæmia and congestion . . . . .	15
Fevers . . . . .	16

	PAGE
Convulsive diseases . . . . .	16
Apoplexy . . . . .	16
Paralysis . . . . .	16
Diseases of the eye . . . . .	16
Cerebral affections . . . . .	17
Chest affections . . . . .	17
Stomach affections . . . . .	17
Disorders of the bladder . . . . .	17
"    "    generative system . . . . .	17

## PART I. DIARRHŒA.

### CHAPTER I.

#### DIARRHŒA ORIGINATED BY HEAT.

Various kinds of diarrhœa : principle of classification . . . . .	21
Summer diarrhœa . . . . .	22
Relation of summer diarrhœa to cholera . . . . .	22
Symptoms of summer diarrhœa . . . . .	25
Proximate cause: Hyperæmia of the spinal and sympathetic nervous centres . . . . .	26
Excessive contraction of the intestinal arteries . . . . .	27
Source of the diarrhœal flux . . . . .	28
Cause of the excessive peristaltic action . . . . .	28
Cause of the general weakness, and of the coolness of the surface of the body, associated with summer diarrhœa . . . . .	29
Prognosis . . . . .	29
Treatment by means of the spinal ice-bag . . . . .	29
How the spinal ice-bag cures diarrhœa . . . . .	30
Directions for selecting a suitable spinal ice-bag in each case . . . . .	31
"    preparing and applying the spinal ice-bag . . . . .	32
The ice-bag jacket . . . . .	33
Necessity of keeping the patient warm . . . . .	34
Medicinal remedies for diarrhœa . . . . .	35
Their efficacy overstated . . . . .	39
Mortality from diarrhœa . . . . .	40
Prophylaxis . . . . .	40
Cases illustrative of treatment by the spinal ice-bag . . . . .	41
Efficacy of the spinal ice-bag superior to that of medicine . . . . .	47
Its soothing and comforting influence . . . . .	47

## CONTENTS.

xiii

### CHAPTER II.

#### DIARRHŒA ORIGINATED BY MOTION.

	PAGE
Proximate cause . . . . .	49
Diarrhœa produced by the motion of ships . . . . .	49
"          "          walking . . . . .	50
Treatment by the spinal ice-bag . . . . .	51

### CHAPTER III.

#### DIARRHŒA ORIGINATED BY (ELECTRICITY OR ITS ANALOGUE) NERVOUS IRRITATION.

Modus operandi of nervous irritation . . . . .	53
Dental irritation . . . . .	54
Irritating ingesta . . . . .	54
Bad water . . . . .	54
Noxious effluvia . . . . .	55
Diseased intestinal glands . . . . .	55
Predisposing, proximate, and exciting causes . . . . .	55
Treatment by the spinal ice-bag . . . . .	56

### CHAPTER IV.

#### DIARRHŒA ORIGINATED BY THE MIND.

Examples . . . . .	57
Proximate cause . . . . .	58
Treatment by the spinal ice-bag . . . . .	58
Cases . . . . .	59

---

## PART II. CHOLERA.

### CHAPTER I.

#### DEFINITION, HISTORY, AND SYMPTOMS.

Definition . . . . .	66
History . . . . .	66

	PAGE
Symptoms . . . . .	68
First stage: Invasion . . . . .	69
Second stage: Development . . . . .	70
Third stage: Collapse . . . . .	72
Fourth stage: Reaction . . . . .	74
Various sequelæ . . . . .	74

## CHAPTER II.

POST-MORTEM PHENOMENA IN CASES OF DEATH DURING  
CHOLERAIC COLLAPSE.

Rise of temperature of the body immediately after death . . . . .	75
Long continuance of bodily heat after death . . . . .	75
Change in the aspect of the skin, which becomes of lighter colour . . . . .	76
Muscular contractions . . . . .	76
Rapid supervention of rigor mortis . . . . .	77
Its duration . . . . .	77
Putrefactive changes . . . . .	77

*Condition of the several parts of the Body:*

The brain . . . . .	77
The spinal cord . . . . .	78
The sympathetic nerve . . . . .	78
Larynx and trachea . . . . .	78
The lungs . . . . .	78
" heart . . . . .	79
" liver . . . . .	79
" pancreas . . . . .	79
" spleen . . . . .	79
" kidneys . . . . .	80
" urinary bladder . . . . .	80
" reproductive organs . . . . .	80
" pharynx and œsophagus . . . . .	80
" stomach . . . . .	81
" peritoneum . . . . .	81
" small intestines . . . . .	81
" intestinal villi . . . . .	81
" glands of the small intestines . . . . .	82
" patches of Peyer . . . . .	82
" mesenteric glands . . . . .	83
" large intestine . . . . .	83
" solitary glands of the large intestine . . . . .	83
Contents of the alimentary canal and the evacuations . . . . .	83
The distribution of the blood . . . . .	85
State of the blood . . . . .	85

# CONTENTS.

xv

## CHAPTER III.

### PATHOLOGY OF CHOLERA.

	PAGE
No satisfactory theory has yet been propounded . . . . .	89
What a satisfactory theory implies . . . . .	89
Doctrine that the sympathetic nervous system is depressed or exhausted . . . . .	90
Alleged influence of the "cholera poison" in paralysing the heart . . . . .	90
Confutation of this theory by Professor Parkes . . . . .	92
Professor Parkes' theory . . . . .	92
Dr. George Johnson's theory . . . . .	93
His assertion of the presence of a blood-poison unproven . . . . .	93
„ mode of accounting for choleraic cramps unscientific . . . . .	93
„ hypothesis that an irritant poison causes the pulmonary artery to contract unduly not proven . . . . .	94
„ explanation of the imperfect aëration of the blood, the fall of temperature, and the suppression of bile and urine, fallacious . . . . .	96
Phenomena of collapse unexplained and ignored by Dr. Johnson . . . . .	97
Value of Dr. Johnson's theory tested by practice . . . . .	98
Existence of a cholera poison questionable . . . . .	99
Its mode of action, if it exists . . . . .	99
First impressions of physicians in India concerning the nature of cholera the truest . . . . .	99
All the phenomena of cholera are due to hyperæmia of the spinal and sympathetic nervous systems . . . . .	99
Classification of phenomena into positive and negative—a list of each . . . . .	100

#### *Explanation of how each Symptom is Produced—Positive Phenomena:*

Abnormally copious and pale urine . . . . .	101
Albuminous urine . . . . .	101
Superabundant secretion of bile . . . . .	101
Superabundant secretion of pancreatic juice . . . . .	102
Borborygmi . . . . .	102
Excessive activity of the mucous membrane and of all the glands of the alimentary canal . . . . .	102
Abnormally high temperature within the rectum . . . . .	106
Excessive activity of the mucous membrane of the gall ducts and gall bladder, of the pelves of the kidneys, and of the female genital organs . . . . .	106
Abdominal gripings . . . . .	107
Excessive expulsive activity of the stomach and bowels . . . . .	107
Simultaneous excessive activity of thoracic and abdominal muscles . . . . .	107



	PAGE
Sweat in all its grades of copiousness and fluidity . . . . .	107
Derangements of sensibility . . . . .	108
Tremors . . . . .	108
Muscular twitchings . . . . .	108
Fixed, stony expression of the face . . . . .	108
Tonic hardness of muscles . . . . .	108
Tightness across lower part of chest . . . . .	109
Cramps and convulsions . . . . .	109
Restlessness and tossing of the limbs to and fro . . . . .	109
Extreme contraction of the urinary bladder . . . . .	109

*Negative Phenomena :*

Slight headache . . . . .	110
Deafness of various grades . . . . .	111
Tinnitus aurium . . . . .	111
Dizziness, slight faintness, syncope . . . . .	111
Drowsiness, sleeplessness . . . . .	111
Mental states . . . . .	111
Non-secretion of tears, saliva, bile, and urine . . . . .	112
Short, struggling, and rapid respiration . . . . .	114
Cold breath . . . . .	116
Enfeeblement of the voice, aphonia . . . . .	116
Oppression and burning pain at precordia and left epigastric region .	116
Algid symptoms: Progressive changes in the visage, and in the colour, temperature, and general aspect of the skin . . . . .	117
Loss of cutaneous sensibility . . . . .	119
Serous exudation into the intestines . . . . .	120
Epithelial exfoliation of the intestinal villi . . . . .	120
Enfeeblement and death of the voluntary muscles . . . . .	122
Enfeeblement and death of the involuntary muscles, cessation of discharges, secretion still continuing . . . . .	122

*Phenomena not included in the foregoing Lists :*

Death of the nervous system . . . . .	123
The increase, or long persistence of heat in the body after death . .	124
Post-mortem changes in the aspect of the skin . . . . .	124
„ muscular contractions . . . . .	124
The early onset of rigor mortis . . . . .	125
Additional evidence from post-mortem appearances . . . . .	125
Epidemic attacks of cramps and spasms . . . . .	125
Differences in the relative activity of the positive and negative motor nerves in different cases . . . . .	126
The stage of reaction . . . . .	129
„ analogy between cholera and sea-sickness . . . . .	130

# CONTENTS.

xvii

## CHAPTER IV.

### CAUSES OF CHOLERA.

	PAGE
Classification of the causes of cholera . . . . .	135
Solar heat . . . . .	136
Wide ranges of temperature . . . . .	138
Atmospheric electricity . . . . .	146
Deficiency of ozone . . . . .	151
Malaria . . . . .	153
Lowness of site . . . . .	156
Prolonged marches, pilgrimages, and ordinary travelling on foot . . . . .	157
Noxious effluvia . . . . .	160
Impure water . . . . .	161
Bad food, and eating to excess . . . . .	162
Dental irritation . . . . .	164
Purgative medicines . . . . .	164
Opium . . . . .	167
Alcoholic fluids . . . . .	168
Habitual excitants of the nervous system . . . . .	169
Mental emotion . . . . .	171
Increased activity of the choleraic influence during the night . . . . .	172
The tendency of cholera to recur in the same individual, and in the same nation . . . . .	174

## CHAPTER V.

### TREATMENT.

SECTION I.—TREATMENT BY COLD AND HEAT . . . . .	179
The spinal ice-bag . . . . .	180
Its efficacy in the treatment of various disorders, the symptoms of which are analogous to those of cholera . . . . .	180
Necessity for the simultaneous application of heat . . . . .	182
Proofs of the curative power of heat . . . . .	183
Various plans of applying heat . . . . .	184
Special directions for applying the spinal ice-bag . . . . .	184
" " " " heat . . . . .	187
Precautions to be observed . . . . .	187
Hot fluids internally . . . . .	188
The astringent influence of brandy . . . . .	190
What drugs are to be especially avoided . . . . .	191
Necessity of giving as much nourishment as possible . . . . .	191

	PAGE
Treatment of the stage of reaction . . . . .	191
Precautions to be observed in applying heat to the spine . . .	192
Good nursing indispensable . . . . .	193
SECTION II.—TREATMENT BY MEDICINES . . . . .	193
Results of treatment by different classes of medicines . . . .	194
Calomel . . . . .	194
Opium . . . . .	195
Sulphuric acid . . . . .	204
The kind of astringents needed . . . . .	205
Bromide of potassium, and bromide of ammonium . . . . .	206
Quinine . . . . .	206
Arsenic . . . . .	207
Nitrate of amyle . . . . .	207

## CHAPTER VI.

## CASES AND RESULTS.

Case of sporadic cholera . . . . .	209
Seven cases of epidemic cholera treated at Southampton in October, 1865 . . . . .	210
Cases of uterine hæmorrhage during collapse . . . . .	218
Professor Parkes' official account of the treatment of cholera by ice at Southampton in 1865 . . . . .	220
Opinions of medical men at Southampton on the efficacy of the ice treatment of cholera . . . . .	222
Cases of cholera treated by ice at Paris . . . . .	224
Case of cholera treated by cold and heat to the spine near Calcutta . . . . .	227
Cases treated in Southampton during July, 1866 . . . . .	229
Summary of results at Southampton during July, 1866 . . . . .	236
Erroneous official reports of deaths following ice treatment at Southampton in July, 1866 . . . . .	238
Opinions of Mr. Bencraft and Dr. Griffin of the results of treatment at Southampton in July, 1866 . . . . .	239
Cases treated at Liverpool by ice alternated with hot-water bags . .	239
Rumoured results of treatment by ice at New York and Brooklyn . .	240
Dr. Davidson's report of the effects of ice along the spine in the treatment of cases of virulent diarrhœa on the Ganges . . . .	240
Cases of choleraic diarrhœa treated successfully by the spinal ice-bag . . . . .	241
Reasons why the treatment of cholera by the spinal ice-bag has not been brought forward in London during the epidemic of 1866 . .	243

## CONTENTS.

xix

	PAGE
Reasons for beginning the ice treatment with cases of diarrhoea and cholera . . . . .	244
Conclusions . . . . .	245

---

## NOTES.

1. Prevention of cholera . . . . .	246
2. Post-mortem state of the sympathetic system and the pneumo- gastric nerves in cases of death during choleraic collapse . .	246
3. Spine-bags . . . . .	247



# INTRODUCTION.

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## SECTION I.

### NEURO-PHYSIOLOGY.

CHOLERA, of all known disorders affecting the human body, affords the most signal example of a disease, the phenomena of which consist in a derangement of the circulatory system. It has been observed by Dr. Gull, that these phenomena, during the period of collapse, are in a pre-eminent degree those of "passiveness."\* There is not only no inflammation, but, excepting the symptoms of excessive activity in the nervous and glandular systems, every feature of the malady is of a directly opposite character: the vital activity is suddenly or speedily arrested, and textural nutrition declines throughout the greater part of the body, the blood being mainly confined within the great vessels. Inflammation itself has, however, with good reason been defined by Dr. Todd as "deranged nutrition;" and thus it appears that the phenomena of cholera, and those of inflammation, though of a diametrically opposite character, and whatever may be their causes, consist alike of disorders of the vascular system—the circulation being, in the one case, reduced to a minimum, and in the other increased to a maximum degree. Between these two extremes of defect and excess, in the local or general supply of blood to the organic tissues, the great majority of diseases to which the human body is liable may be ranged; for they may be shown to consist in some excessive or defective activity of the processes of textural nutrition, and of the blood currents ministering to those processes. Of course these various disturbances of nutrition, and of the functions of the vascular system, may be expressive of a great variety of causes, and notably of the absorption of poisons; those, for example, of small-pox, scarlatina, and intermittent fever: for whatever may be the character of each special disease, and the nature of its efficient cause, the resulting phenomena are always chiefly observable as

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\* "Cholera appears to consist of but one single series of actions, which may vary in intensity through every gradation, but throughout maintain the same character of passiveness."—"Report on the Morbid Anatomy, Pathology, and Treatment of Epidemic Cholera," by W. G. Gull, M.D., page 121.

disorders either local or general of textural metamorphosis and of the circulatory system. Moreover, the remedial efforts of the physician are mainly directed to subdue these disorders. He battles with symptoms : he knows nothing of the essential nature of the great majority of those blood-poisons whose effects constitute disease ; he has never seen them ; he cannot isolate them ; he cannot eliminate them ; by their extreme subtlety they elude his apprehension, and, fascinating his imagination, tempt him to ascribe to their agency diseases not actually due to them. When they have once gained access to the body, the utmost he can do is to combat or restrain their activity, and counteract or lessen their effects.

The more, however, these effects are studied, the more convinced pathologists become that they are immediately dependent on morbid states of the nervous system, which is primarily affected—its departure from health being first denoted by a disorder of its own circulation, which subsequently originates those derangements of circulation of various kinds in various parts of the body, variously denominated as special diseases, according to their localities and peculiarities.

Now, if the foregoing statements be true, it is obvious that in proportion as the physician can exercise an accelerating and restraining power over the circulation of the blood, and especially over its circulation in the nervous centres, he will acquire a remedial control over a large majority of the diseases of the human body. This accelerating and restraining power I claim to have discovered, and have had abundant experience that it may be effectively exercised by means of cold and heat—its influence being exerted *primarily* over the nervous centres, and *secondarily*, through them, over the circulation and functions of the other organs of the body.

The eminent physiologist, Professor Claude Bernard, when making experimental researches into the influence of the nervous system on the production of animal heat, discovered that, when he divided the sympathetic nerve on one side of the neck of a rabbit, the temperature of the corresponding side of the head became considerably higher than that of the opposite side. He found, also, that the circulation and functional activity of the parts affected were increased, and that when the animal is killed, the reflex faculty lasts longer there than in the other side. It was afterwards ascertained by Brown-Séquard that cadaveric rigidity comes on later and lasts longer, and that putrefaction begins less speedily than on the other side. Further investigation proved that, by galvanizing the cut end of the upper part of the nerve in question, phenomena, the exact converse of those just mentioned, are induced : the circulation in the corresponding side of the head is lessened, the part becomes colder, its vitality is impaired, and when the animal is killed the vital properties of the motor and sensitive nerves disappear there sooner than on the other side ; cadaveric rigidity also comes sooner and lasts less time, and putrefaction sets in sooner

than on the other side. These opposite groups of results prove that the branches of the sympathetic nerve distributed to the head exercise a supreme controlling influence over its blood-vessels. Further investigations have demonstrated that a like relation to that between the sympathetic and the blood-vessels of the head obtains between the other segments of that nerve and the vascular system of the rest of the body. The sympathetic nerve is therefore the motor nerve of the blood-vessels throughout the body. It has long been thought that one of the chief functions of this nerve is to preside over and regulate the activity of the glandular system. But increasing physiological knowledge shows more and more clearly that it does so only in the same way as it presides over all other organs, viz., by regulating their blood-supply. It may, I believe, be confidently stated that the only positive knowledge yet obtained respecting the functions of the sympathetic proves that its action consists in stimulating the muscular coats of the blood-vessels, and thus in modifying the volume of the blood currents. When the sympathetic ganglia are largely supplied with blood their functional activity is proportionately intensified; they therefore emit more energetic currents along the nerves branching from them, and distributed to the muscular coats of the blood-vessels to which they are severally related; the bands of muscular fibres constituting this coat being thus stimulated, contract, and so lessen the diameters of the vessels they surround. The consequence is, that the volumes of the blood currents passing through them are lessened, and that the structures nourished by those currents, having a portion of their supply cut off, become less active or less vital than before: textural change, the evolution of heat, and the functions of the organs in question, decline.

If, on the other hand, the circulation in, and therefore the nutrition and functional power of, the sympathetic ganglia be lessened, the nerves distributed from them to the blood-vessels to which they are related, convey less energetic currents to the muscular coats of those vessels than before. Those coats, therefore, become proportionately relaxed; and the blood, flowing in the direction of least resistance in proportionately increased volumes, distends these vessels and supplies the structures amid which they ramify more copiously than before. The consequence is, that the nutrition, temperature, and functions—in short, all the vital properties—of these structures are correspondingly increased.

Considering the physiological relations thus said to obtain between the sympathetic ganglia and the blood-vessels throughout the body, it is evident that, if it were possible to lessen or increase the circulation of the blood in those ganglia, it would be possible, secondarily, through their agency, to increase or lessen the circulation in every part of the body.

In 1863, I published the discovery which I had made, that the circulation in those ganglia could be lessened by the application of



ice, and could be increased by the application of heat along the region of the spine. My experience since that time has abundantly proved the reality of the discovery; and I can state that the circulation in the head, and in the thoracic, the abdominal and pelvic viscera, can be increased or lessened by modifying the temperature of the appropriate segments of the back. I also stated, in 1863, that, for the reason then adduced, ice can be safely applied for long periods of time along the spine; that, therefore, the circulation of the blood in the spinal cord could be increased or lessened in the same way; and that this discovery confers the power of modifying the nervous currents to the voluntary muscles, and thus of exercising a remedial influence over their morbid conditions, and notably over those of a spasmodic or convulsive character.

Second only in importance to the power here described of increasing or decreasing the general circulation, through the agency of the nervous centres, is the power, I have also discovered, by which the glandular system—especially of the skin and of the mucous membranes—may be influenced in like manner, but with this very remarkable difference, viz., that whereas the peripheral circulation is increased by cold and decreased by heat along the spine, the activity of the glandular system is decreased by cold and increased by heat along the spine. These results involve a seemingly astonishing paradox; for, when heat is applied along the spine, and when, consequently, as I allege, the glands are stimulated most vigorously, the vaso-motor nerve centres are emitting their maximum of energy, and are thus shutting off a large proportion of the blood which otherwise would be passing through the peripheral arteries. Hence it seems, at first sight, as if glands secrete most copiously at the very time when blood—the source of their products—is flowing to them least copiously. The following facts which I have ascertained will illustrate this statement:—

If in a case of feverishness, the skin being hot and dry, heat be applied along the spine, the skin will become moist and comparatively cool *simultaneously*. If, during the first stage of bronchitis, when there is an irritative cough, when the bronchial mucous membrane is void of mucus and is dry, and when there is dyspnoea with pain and a sense of fulness in the chest, a double column of heat be applied between the scapulæ, the irritative cough will subside, bland mucus will be secreted, the painful sense of fulness and difficulty of breathing will be relieved, and the thoracic integuments, previously warmer, will become cooler than normal. Again, if heat be applied along the lower half of the dorsal spines, the usual restraining influence on the peripheral circulation will be exerted; and meanwhile, in many cases, nausea, and in some cases vomiting even, will be induced. And again, if heat be applied along both the lower dorsal and the lumbar spines, the bowels will be deprived of their wonted supply of blood, and, in some cases, the amount of

mucus poured out from the intestinal mucous membrane will be so copious as to induce diarrhœa.

And contrariwise: if in a case of so-called general relaxation, associated with a cold, unduly moist (clammy) skin, ice be applied along the whole spine, the system will experience a tonic influence, and simultaneously the skin will, as a general rule, become both warmer and drier. If, during the later stages of bronchitis, when all inflammatory symptoms have subsided and mucus is exuded and coughed up in great abundance, ice be applied between the scapulæ, the temperature of the thorax will be raised and the secretion of mucus will be lessened or arrested. Analogous results may be obtained by the same means in each segment of the body. During severe sea-sickness the skin is cold and is covered with perspiration, mucus is vomited from the stomach after whatever food it may have contained has been ejected, and diarrhœa often occurs.

Now the condition described in the first of the two preceding paragraphs as induced by heat, and in the second as restrained or subdued by cold, is twofold: in the first case, it consists in a contraction or collapse of blood-vessels, and simultaneously in an excessive outpouring from the glandular organs—especially from those of the mucous membranes and of the skin; in the second case, it consists in a dilatation of blood-vessels, and in a diminution of the activity of the glandular organs—especially of those of the mucous membranes and of the skin.

It cannot be said that one of these conditions being induced the other necessarily occurs, for they may be induced separately: when the surface of the body is exposed to a high temperature, profuse sweating occurs, but a maximum amount of blood circulates in the skin, the blood-vessels of which therefore are dilated. On the other hand, when the surface of the body is exposed to a low temperature, the blood-vessels of the skin are contracted, and a minimum amount of blood is circulating in them, but no perceptible sweat whatever is exuded. If, however, instead of modifying the temperature of the *surface* of the body, the nervous centres along the spine be acted upon, both the conditions in question are then induced, and are inseparable; cutaneous anæmia, and cool, or cold sweat occur together; and in like manner anæmia of the inner skin—the mucous membrane—and a copious exudation of mucus simultaneously take place. While, therefore, it is clear that cutaneous anæmia and perspiration on the one hand, and anæmia of the mucous membranes and free exudation of mucus on the other, do not respectively stand in a causal relation to each other, it is not less clear that each is a result of one common cause, viz., hyperæmia of the nervous centres along the spine.

The paradox here advanced, that the skin and mucous membrane exude their appropriate secretions copiously at the very time when they are receiving blood—the source of those secretions—least copiously, is itself extremely surprising, and, at first sight, difficult to

understand ; but the surprise and difficulty are greatly increased of those who hold the commonly received doctrine that the secreting processes are mainly under the direction and control of the sympathetic ; for that doctrine implies that the influence thus exerted upon the various glands consists chiefly, at all events, in regulating their blood-supply, and that, therefore, their activity and products are increased by permitting their arteries to dilate, and are lessened by causing them to contract. Whereas it appears that while the contractile energy transmitted to the gland-arteries from the sympathetic ganglia is at its maximum, the glands are active, and pour out their products freely.

The question, How does the mucons membrane secrete mucus and the skin sweat abundantly while supplied with a minimum amount of blood ? is as interesting and important as it seems at first sight difficult to answer. I shall, however, propose a hypothesis by way of provisional explanation of the phenomena—a hypothesis which, in the present state of our knowledge, is undoubtedly liable to be confuted, but which, inasmuch as it accounts for the facts in question, will, perhaps, be held worthy of acceptance until disproved, and will meantime indicate the direction and shape the course of further inquiry. The above-mentioned facts, and others to which I shall briefly refer, lead me to the conclusion that *all* secreting organs are supplied with nerves acting directly upon their constituent cells, which become active or passive according to the amount of nervous influence distributed to them ; that their elective affinity for the particular elements of the blood which they separate from it, and the force of their attraction for blood on which to operate, are exclusively due to and dependent on that influence ; that when that influence is exerted in a maximum degree, the attractive force exerted by the gland-cells on the blood is so great that it counteracts and neutralizes the contractile force of the muscular coat of the arteries from which they derive the blood they require, and therefore, that while hyperæmia of the nervous centres lessens the general blood-supply to the periphery of the body, by causing contraction or tonic spasm of the peripheral arteries, that same hyperæmia generates and transmits to the gland-cells a preternaturally strong nervous influence, enabling them to draw copious currents of blood to themselves through their special arteries, notwithstanding that they are at the same time subject to the maximum of contractile force, and that the peripheral arteries generally are in a condition of anæmic spasm.

In my pamphlet on sea-sickness, published in 1864, I said, “ It may be that the sweat-glands are under the control of a special set of nerves as distinct in their functions as those presiding over the circulatory system. Their action, however, may be very different. The stimulus of a vaso-motor nerve, as we know, causes the vessel on which it ramifies to contract ; but the stimulus transmitted along the nerves presumed to regulate the action of the sweat-glands

may cause them to pour out their secretion in increased abundance. If so, it is intelligible how it comes to pass that when there is a maximum quantity of blood in the sympathetic nervous centres, induced in cases of sea-sickness, as already explained, perspiration is profuse, although there is a minimum amount of blood in the skin. I am tempted to make this suggestion, because when in treating cases of an inflammatory character, the pulse being high, and the skin hot and dry, I have applied heat along the back, not only has the pulse been reduced, the inflammatory symptoms subdued, and the skin become cool, as I predicted and expected, but, unexpectedly, the patients have also speedily broken out into perspiration. In these cases it is certain that the nervous centres along the back had an unusual amount of blood drawn into them by the heat which had been applied, and that, therefore, they were, physiologically speaking, in the same state as are the nervous centres of persons suffering from sea-sickness, and in both classes perspiration obtains. I have already adverted to the great sympathy or intimate relation between the skin and the mucous membrane of the alimentary canal, indeed there is great analogy between their functions; and bearing on this question of the physiology of sweating, I have observed a fact in respect to the bronchial mucous membrane similar to that which I have just recorded in reference to the skin, and of an equally unexpected character. In treating pulmonary catarrh and bronchitis, I apply heat along the dorsal region, in order, by inducing a preternatural afflux of blood in the thoracic ganglia, to cause the arteries supplying the bronchial mucous membrane to contract. Now, if at the stage of congestion of this membrane, when it is dry, its wonted secretion being absent, heat be applied to the dorsal region, as described, not only will the congestion be lessened, as I expected, but, contrary to what I at first expected, a secretion of mucus rapidly ensues, to the great relief of the patient. It seems to me that this phenomenon can only be accounted for on some such principle as that suggested to account for the perspiration also induced as described by the application of heat to the back. These suggestions receive considerable countenance from the fact announced by Prof. Claude Bernard—that the salivary glands are not under the control of the sympathetic, but of the lingual nerve, and that their secretion increases in proportion to the amount of positive stimulus received by them from that nerve. It seems to me probable that what I have just said will explain how the sweating, sickness, and diarrhoea of phthisis are induced, and will suggest how it may be lessened or prevented. The sympathetic ganglia are, I believe, brought into a state of hyperæmia by the irritation of the vasic nerves distributed to those blood-vessels in the vicinity of tubercular deposits; and this hyperæmia becomes the proximate cause of the phenomena in question."

When I wrote the foregoing paragraph, the only knowledge I

had of Bernard's discovery respecting the salivary glands was derived from the following statement by Brown-Séquard :—"The researches of Czermak and of Professor Bernard tend to show that the increase in the salivary secretion does not depend on the sympathetic nerve, but on the lingual." I have since had the good fortune to become personally acquainted with Professor Bernard, who kindly presented to me copies of several monographs in which his discoveries were communicated to the Academy of Sciences, and from these I have obtained the most satisfactory confirmation of the views I have expressed respecting the innervation of the glands of the mucous membranes and of the skin.

Bernard has proved that the submaxillary and parotid glands are each supplied with a special motor nerve, emanating from the cerebro-spinal system, as well as with the branches of the sympathetic supplied to the arteries of the glands. He has also proved that when the cerebro-spinal nerve is excited, the gland becomes active, that the amount of blood which then passes through the gland is greatly increased, that the colour of the blood flowing through the glandular vein becomes red, and that the amount of saliva secreted is proportionate to the irritation of the cerebro-spinal nerve; also, that if the branches of the sympathetic nerve distributed to the gland be irritated, the supply of blood to it is almost wholly cut off, that the salivary secretion is arrested, and finally, that when the same branches are divided the supply of blood to the gland and the secretion of saliva become copious. The special motor nerve of the submaxillary gland is the chorda tympani; that of the parotid gland consists of a branch or branches from the auriculo-temporal nerve.\* For the sake of convenience, I shall henceforth call the special motor-nerve of each gland *the positive motor-nerve*, and the branch of the sympathetic distributed to the glandular artery, *the negative motor*.

Bernard himself infers from the facts just mentioned, that a like structural and physiological arrangement obtains in relation to glands generally, and has proved that when the kidneys are active, red blood flows from the renal vein. Thus my own discoveries with respect to the action of cold and heat along the spine on the secretion of sweat and mucus, harmonize with his respecting the innervation of the salivary and parotid glands; while my inference that special cerebro-spinal (positive motor) nerves preside over the functions of the mucous and sweat glands, receives the highest authoritative and scientific sanction, inasmuch as Bernard's discoveries, respecting the salivary glands, led him to a conclusion similar to mine concerning the general functional relation of the nervous to the glandular system.

There is, however, one important question connected with the innervation of glands on which I am constrained to differ from

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\* Ludwig has also instituted experimental researches into the innervation of these glands: his results are in general accord with those of Bernard.

Professor Bernard. That question is,—How, in the case of the sub-maxillary gland, does the chorda tympani nerve promote the secretion of saliva? In other words,—what is the exact mode of action of the nerve? Bernard seems to believe that the gland will secrete saliva if only it be supplied with blood, and expresses the opinion that the function of the chorda tympani nerve is to neutralize or disable, by a sort of paralysis, the branches of the sympathetic supplied to the arteries of the gland, so that when saliva is needed they are enabled to dilate, and thus to allow a maximum amount of blood to pass through them to the gland. Ever since my attention has been directed to this subject, I have been accustomed to consider that the gland textures are under the immediate stimulating influence of the nerves distributed to them, that in fact the constituent elements of the gland are endowed with the power of attracting the blood they require from which to elaborate their secretions, by being placed and sustained in a certain electric state through the agency of the nerves distributed to them. This mode of conceiving of the character of the immediate action of the nerves on the glands seems to me to offer an adequate explanation of the phenomenon in question, and at the same time to accord most completely with all we know concerning the way in which nerves terminate in the various structures to which they are related. Moreover, while the argument from analogy leads to this conclusion, physiological knowledge affords, I believe, no precedent in favour of the doctrine propounded by Bernard. My profound respect for this great physiologist would make it very difficult for me to espouse a theory which he rejects, were I able to support it only by *à priori* reasoning, and considerations of analogy; but, fortunately, I am saved the necessity of resting exclusively upon these, for Pfüger, who has just published the results of his laborious microscopical investigations respecting the mode of ending of the nerve filaments ramifying in the submaxillary glands,\* has seemingly settled the question in the most decisive manner: he proves that the filaments of the chorda tympani nerve are distributed to the gland cells, and not only so, but that their several terminations are actually in the nuclei of the cells. The wondrous care and patience, as well as the philosophical spirit, with which these researches have been conducted, and the minute descriptions, illustrated by plates, which he has given of his observations, leave upon the mind of the reader the impression that the statements of this physiologist are faithful reports of actual facts. The remarkable loyalty to truth which distinguishes Bernard pre-eminently, assures me that he will be among the first to recognise the importance of Pfüger's observations, and, in deference to the facts they reveal, will abandon without hesitation the opinion which he has entertained on this question.

The innervation of glands is, in my opinion, a subject of such

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\* Die Endigungen der Absonderungsnerven in den Speicheldrüsen. Von Dr. A. F. W. Pfüger. Bonn, 1866.

paramount importance in its bearing on the object of this book, that I shall insert here a summary of the present state of knowledge, and of the desiderata of this branch of neuro-physiology, extracted from a manuscript which I hope soon to publish.

"1. We know that sweat is exuded when the nervous centres along the spine have become hyperæmic, either by pathological influences or the application of heat; and that cold, on the contrary, restrains its secretion. We have, therefore, the strongest reasons for concluding that the sweat glands are provided with centrifugal nerves from the cerebro-spinal axis: anatomical proof that they are thus provided is a desideratum.

"2. We know that the secretion of mucus in each mucous membrane is promoted or retarded by the same conditions as is the secretion of sweat. We conclude, therefore, that the mucous glands are also provided with centrifugal nerves from the cerebro-spinal axis: anatomical proof that they are so is, however, still wanting.

"3. The scientific demonstration of the nature of the innervation of the submaxillary and parotid glands by Bernard, and of the mode of termination of the nerve in the cells of the submaxillary gland by Pflüger, while leaving nothing to be desired in respect to them, is in perfect accord with my observations that congestion or hyperæmia of the cerebro-spinal nervous centres is often, in adults at least, associated with a copious flow of saliva, as well as with the well-known fact that the hyperæmia of the medulla oblongata—often extending to the brain and spinal cord—induced by the irritation of teething is the proximate cause of the copious flow of saliva in children. I am not aware if any experiments on the sub-lingual gland like to those of Bernard on the parotid and submaxillary have yet been made; but after carefully examining into the anatomical relation of the nerves supplied to the sublingual, I feel quite certain that those relations are identical in principle with those discovered in respect to the parotid and submaxillary: the gland is supplied by branches from the gustatory nerve (which is of course sensory), but before supplying the gland, this nerve receives filaments from the ninth or hypoglossal which is motor, and these filaments, or most of them, constitute, as I conceive, the positive motor-nerve of the gland. It may be, however, that some of these filaments co-operate with those from the facial to innervate the mucous glands of the tongue.

"4. The lachrymal gland is, as I have already proved by anatomical evidence, supplied with a centrifugal nerve from the brain as well as from the sympathetic, but physiological or pathological evidence of the function of the former is still wanting. Of course it is all but certain that the function of the cerebro-centrifugal nerve filament supplied to the gland, whether from the motor oculi or the patheticus is identical with that of the chorda tympani in relation to the submaxillary gland. This presumption is strengthened by the fact, that in many cases of sudden emotion,

especially during childhood, tears are shed instantaneously; for as the action of the sympathetic is notably slower than that of the cerebro-spinal system, the conclusion seems inevitable that the path of emotional influence to the gland is that of the cerebral nerve, and generally that of the filament from the fourth.

"5. The spinal nerves, distributed to the mammary glands, of course contain motor fibres; but that these fibres constitute, as I have no doubt they do, the positive motor nerves of the glands requires to be ascertained. This demonstration could, I imagine, be easily obtained by dividing the anterior roots of the spinal nerves distributed to the mammae of an animal at the time it is giving suck.

"6. The innervation of the liver is highly complex; for the hepatic plexus, derived from the solar plexus, which is itself derived from several sources, receives filaments also from the left pneumogastric and the right phrenic nerves. Whatever may be the function of the pneumogastric, the phrenic is certainly motor, and therefore of itself fulfils the requirement of the hypothesis, that every gland is supplied with a centrifugal nerve from the cerebro-spinal axis. In respect to the liver, this hypothesis receives further confirmation from the fact discovered by Bernard, that a puncture, or slight irritation of the floor of the fourth ventricle, will cause sugar to appear in the urine, while a more severe injury will restrain the elimination of sugar, seemingly by stopping its production. Without hazarding an opinion as to the result, I may suggest that, were it practicable to divide the branch of the phrenic nerve supplied to the hepatic plexus, without otherwise injuring the innervation of the liver, and then to observe the effects on the activity of the organ, the experiment would prove instructive.

"7. The pancreas also derives its nerves chiefly from the hepatic plexus; and therefore the observations just made apply to it as well as to the liver.

"8. The as yet unsolved problems concerning the innervation of the stomach are too well known, and have taxed the ingenuity of too many eminent physiologists, to need mention here. I may say, however, that my experiments with cold and heat along the spine have satisfied me that the mucous glands of the stomach fulfil their office at the bidding of centrifugal nerves from the spinal cord: anatomical demonstration of this fact is wanting, but I do not see how it can be supplied. As already stated, there is abundant evidence that the functions of the stomach are under the immediate influence of the cerebro-spinal axis, but what are the positive motor nerves which cause the secretion of the gastric juice, remains, I believe, to be discovered. Moreover, much as the pneumogastric in its relation to the stomach, as well as to the abdominal viscera generally, has been studied, its functions are still extremely obscure.

"9. The mucous glands of the intestines have been already referred to implicitly in the general statements I have made concerning the innervation of mucous membranes. If there are, as is believed,



intestinal glands which contribute substances other than mucus to the alvine discharges, these glands may still be grouped with the intestinal mucous glands, so far as the question of their innervation is concerned. Anatomy proves that all the abdominal nervous plexuses receive filaments from the cerebro-spinal system, although it can point to no special motor filaments from that system distributed directly to the intestines. Anatomical evidence of the dependence of the glands, as well as the muscular coat of the intestines on spinal motor nerves, is a desideratum; but physiological and pathological evidence of it abounds; while therapeutical also is not wanting. The most striking instances of the latter kind consist in the arrest of intestinal flux by the application of ice along the spine.

"10. I have convinced myself, by numerous observations, that hyperæmia of the spinal cord is the proximate cause of the copious flow of pale urine so common in cases of so-called [and generally miscalled] hysteria, and in other affections of the nervous system. In fact, abnormally copious and pale urine is such a frequent symptom or concomitant of nervous disease, that according to my experience, it is met with quite as often in male as in female patients, whose nervous systems are disordered. I have met with numerous cases of persons, otherwise in fair average health, who are troubled with an extraordinary great and rapid increase of the quantity of urine secreted whenever they experience any mental excitement. Opium produces a like effect. I have, moreover, proved, to my own satisfaction at least, that the quantity and quality of the urine can be modified by modifying the temperature, and hence the vascular condition, of the appropriate segments of the spinal cord. These facts I have long been in the habit of regarding as significant that the kidneys derive from it centrifugal nerves, which preside over their secretory functions; and though, if it ever be possible, the positive motor nerves of the kidneys have yet to be demonstrated, Bernard has already shown that, when the kidneys are active, red blood flows from the renal veins—a fact going far to prove that the innervation of the kidneys is identical with that of the salivary glands.

"11. The generative glands—the testicles and ovaries—are like the kidneys, innervated from a composite plexus, the contributing motor nerve filaments of which anatomy may be powerless to demonstrate, although it has proved, as already said, that the group of enteric nervous centres, of which the spermatic plexus is a member, contain cerebro-spinal fibres. Physiology and pathology have, however, completely established an intimate functional relation between the spinal cord and male generative organs; and I possess the most indubitable evidence of being able to exert a therapeutical influence through the agency of the spinal cord over the testicles, and in a less obvious, though clearly appreciable  
"or the ovaries also."

The foregoing summary will be admitted, I presume, to present convincing evidence that the disposition of the nervous system is such as to enable every gland in the body, whether cutaneous, or mucous, or special, to derive a positive motor nerve from some part of the cerebro-spinal axis; for it shows that two glands have been proved to possess cerebral nerves acting as motors: that the lachrymal derives a motor filament from the fourth or third nerve, or from both; that the sublingual derives branches from the ninth; that the mammæ are innervated direct from the spinal cord; and that, in every case in which glands are not yet proved by anatomical evidence to possess positive motor nerves from the cerebro-spinal system, physiological, pathological, and often therapeutical facts, prove that they must be so innervated, while anatomy offers no evidence whatever to the contrary. The therapeutical consequences of the establishment of this large anatomical and physiological generalization are unspeakably great, and cannot even be foreseen at once in all their magnitude. Each physician can, however, think out for himself the extent of the therapeutically revolutionary doctrine involved in the statement, that diseases of the skin and of each mucous membrane, to the extent at least to which their glands and their blood-supply are concerned, are, according to the evidence already tendered, nervous diseases, and that the most scientific method of treating them is through the agency of the nervous system.

But it seems to me that the present state of anatomical and physiological science justifies the extension of this generalization still further by enabling us to prove analogically, and also by means of some important facts directly relating to certain structures in question, that the constituent elements of the tissues forming every organ of the body are as completely under the control of the nervous system as is each individual cell of the cutaneous, mucous, or special glands. The limits within which I desire to confine this introduction preclude me from entering at length on this subject here; and as I shall discuss it in the work which I am preparing for publication, and from which the foregoing summary is extracted, I content myself now with stating my conviction that, in the same way as gland-cells are shown to become attractive to the blood supplied to them by virtue of nervous influence exerted directly upon them, so the cells forming all other structures of the organism are endowed with their peculiar elective affinities for those elements of the blood needed for their nutrition and especial purposes by the agency of centrifugal, or positive motor nerves operating directly on the cellular elements of each structure.

Assuming this proposition fairly proveable, we should be able to say of the bodily structures generally that, in addition to the sensory nerve supplied to each, there are two motors: one exercising on the cellular constituents of the structure a stimulant in-

fluence, causing it to attract blood to itself; the other by stimulating the muscular coat of the arteries, causing them to contract, and thus to regulate, restrain, or even to arrest the blood-supply. If this doctrine be at once true and applicable to the whole body, it appears that healthy life, like the perfect movements of the planets, consists in a balance or relative adjustment of two opposing forces; and that, while considerable perturbations of those forces are not inconsistent with health, any excessive subordination of the one to the other induces, or, indeed, constitutes disease, in the structure in which it occurs. Inflammation ending in textural death by excessive and therefore exhausting activity of the reproductive force is induced when the positive motor nerves have become exclusively active; anæmia, also ending in death by mere cessation of the vital processes, is induced when the negative motor nerves are exclusively active. There are few constitutions, I apprehend, in which the relative strength of the positive and negative motor nerves, or, to go further back, the cerebro-spinal and the sympathetic nervous systems, is adjusted to a perfect standard. The greater or less relative strength of each in the organism is, I believe, the ultimate physical fact lying at the foundation and determining the character of, national and individual temperament. The differences in different individuals in this respect account, in my opinion, for the widely different effects which are produced in them by the application of cold or heat along the spine.

I will sum up this section by stating, in a series of brief propositions, the main conclusions which have been arrived at :—

1. That the chief function of the sympathetic nervous system consists in regulating the diameters of the blood-vessels throughout the body.

2. That when the sympathetic ganglia are in a state of maximum hyperæmia the nervous effluence from them to the muscular coats of the arteries to which they are severally related stimulates them so excessively as to induce in them a condition of tonic spasm—a spasm so intense as to result in shutting off the blood altogether from a large proportion of the peripheral arteries.

3. That when the sympathetic ganglia are in a state of maximum anæmia the nervous effluence from them to the muscular coats of the arteries to which they are severally related becomes so extremely feeble that a condition resembling paralysis is induced; the muscular coats of the arteries become consequently extremely relaxed; and, as the blood flows in the direction of least resistance, the parts supplied by the arteries in question become suffused with blood to an excessive degree.

4. That when the spinal cord is in a state of hyperæmia, cramps of the involuntary muscles surrounding the alimentary tube as well as convulsions of the voluntary muscles, anæmia was likely to ensue.

5. That every gland and glandular follicle in the body is under the control of one motor nerve (which I call the *positive motor*) emerging from the cerebro-spinal system, and distributed to its secreting cells in order to regulate its functional activity; and of another motor nerve (which I call the *negative motor*) emerging from the sympathetic system, and distributed to its artery or arterial twig, in order to regulate its blood-supply.

6. That in the same manner as glands are supplied with positive, as well as with negative, motor nerves, so, there is reason to believe, every tissue of the body is thus supplied, and is thus placed and sustained in a state of elective affinity for the elements of the blood requisite for its nourishment and functions.

7. That the sympathetic ganglia and the spinal cord can be rendered hyperæmic or anæmic, artificially, by means of heat, in the one case, and cold in the other, applied along the spine.

8. That by means of heat applied along the spine the general circulation may be lessened, the activity of the glandular system may be increased, and, in some cases, cramps of both the voluntary and involuntary muscles may be induced.

9. That by means of cold applied along the spine the general circulation may be increased, the activity of the glandular system lessened, and cramps of both voluntary and involuntary muscles may be arrested or prevented.

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## SECTION II.

### NEURO-THERAPEUTICS.

THE experience which I have had in treating numerous diseases in accordance with the principles now briefly enunciated encourages me to believe that the remedial method which I have adopted will become generally recognised as at once more powerful, more manageable, and more efficacious than any now in use. I have subdued congestion of various parts of the body on many occasions by applying heat to the appropriate segments of the spinal region. Anæmia, or deficient nourishment of parts (inadequate supply of blood to the brain resulting in extreme cases in fainting fits, for example), may be treated successfully by the application of cold in the same way. I have had but slight experience in the treatment of fever, but I anticipate that fevers of all kinds will be most effectually controlled by cold along the spine in the cold stage, when the blood-vessels are contracted, and heat in the hot, when they are relaxed. I apprehend that the prolonged cold stage of the so-called *fièvre algide*, common on the shores of the Mediterranean, may be cut short by means of ice. Of course in cases of fever due to a zymotic poison this treatment cannot remove the cause; but neither can any known

treatment do so. Cold, in the cold stage, and heat in the hot, will, however, I predict, exercise an effective controlling or restraining influence on all forms of fever, the exanthemata included.

The whole group of convulsive diseases I treat by means of ice along the spine; for evidence of my successful treatment of these diseases I refer the reader to my reports of cases, published on various occasions in *The Medical Times and Gazette*, and more especially to a paper containing a report of several cases of epilepsy, entitled, "Epilepsy, Paralysis, and other Diseases of the Nervous System, treated successfully, chiefly by means of Ice," and published in *The Journal of Mental Science*, for July, 1866.\* Laryngismus stridulus (false croup), and infantile convulsions generally, will be found wonderfully amenable to the spinal ice-bag.

My experience in several cases of apoplexy justifies me in forming the most favourable opinion of the application of heat and cold along the spine as remedial agents in these cases. They have sometimes failed entirely; but in other cases, particulars of some of which I have published, their success has been very remarkable.

Paralysis, when associated with contraction of the muscles implicated, I have most generally found benefited by means of ice; I have, however, treated other forms of paralysis with excellent results. I have succeeded in causing the re-development of muscles to a considerable extent in a very bad case of wasting palsy; and I do not hesitate to predict that if infantile paralysis be treated by means of ice as soon as it occurs, it will be almost always cured.

My own success in improving vision by spinal applications has been not less striking than novel; and Mr. Ernest Hart, complying with my request to make trial of the method I had originated, has already published a report of his successful treatment of a case of that hitherto incurable form of blindness, cerebral amaurosis, and has informed me of two other cases of grave impairment of sight, in which, by the use of spine-bags, he has conferred signal benefit. In the case of amaurosis, consisting of spasmodic contraction of the retinal arteries, they became dilated by means of ice—the retina losing its pale anæmic colour and resuming the pinky hue of health, the vision was correspondingly improved, and, simultaneously, the epileptic attacks, from which the patient had long suffered about three times a week, were so subdued that they occurred only twice during the whole five weeks of treatment. In one of the other cases, in which there was congestion of the retinal vessels, causing intermittent amaurosis, the symptoms, which had long withstood all ordinary treatment, were completely subdued by means of heat to the spine. These facts prove how decisively the cerebral circulation may be influenced by the applications in question, and while encouraging the hope that many cases of impaired vision previously hopeless may hence-

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\* This will soon be published, with additions, in a separate form.

forth come into the category of curable diseases, accord with my experience of many cases in which I have exerted a remedial influence on the brain by spinal applications. Indeed, my success in these cases warrants me in believing that by these applications it will be possible to remedy cerebral diseases, including various forms of insanity, in an unprecedentedly great degree.

My experience of the treatment of chest affections has afforded me the most decisive proofs of the great power which can be exercised over the circulation of the chest-walls and of the lungs themselves. Pleurisy, bronchitis, pulmonary congestion, pulmonary hæmorrhage can be alike restrained or arrested by applications of cold or heat, according to the special needs of the case, between the scapulæ. How much may be done in the several stages of consumption experience has not yet determined, but that *very much* may be, is indubitably implied in the statements just made; and, as I shall hereafter show, the diarrhœa incident to the latter stages of phthisis may be wonderfully restrained by the same method.

The extraordinarily powerful influence of ice along the spine as a remedy of functional diseases of the stomach is attested by its efficacy in arresting sea-sickness and the sickness of pregnancy—two maladies which, up to the date of my discovery how to cure them, have always baffled every attempt to subdue them. But, indeed, nausea and vomiting, from whatever cause, can now, in the great majority of cases, be either completely cured or very greatly lessened; and functional diseases of the stomach generally may be treated, according to my experience in numerous cases, with far more success by modifying the temperature of the spinal region than by any drugs I know of.

Bowel affections I shall have much to say about in future chapters of this book, and therefore will only say here that I have found severe and chronic constipation as remediable by spinal applications, as diarrhœa will be hereafter shown to be.

Of all the maladies which flesh is heir to, none perhaps are at once more troublesome and less amenable to ordinary treatment than those functional disorders of the bladder, due, on the one hand, to enfeeblement of its muscular coat, and therefore of its expulsive power, and on the other, to excessive irritability of its mucous membrane, resulting in an almost incessantly recurring need to micturate. I believe no medicines can be named which avail to overcome either of these distressing conditions; but in case after case which has come before me, and in which one or the other of these conditions has been present, I have succeeded either in effecting a complete cure, or in conferring great benefit, by means of ice.

The functional disorders of the generative system are not less strikingly under the control of cold and heat applied to the appropriate part of the spine, to a degree far surpassing that which can be exerted over them by drugs. I have abundant and completely con-

vincing evidence that those excessively frequent seminal emissions, denoting serious impairment of the health of the spinal cord, can be arrested; that the male organs can be strengthened; and that the numerous derangements of the female organs, grouped together as the functional diseases of women, are capable of being remedied, by cold and heat applied to the spine, to an extent and with a certainty altogether without precedent. That this last assertion is merely a sober statement of fact, any reader may convince himself by referring to my pamphlet, entitled *The Functional Diseases of Women*.

I am aware that the large claims put forward in this section on account of the remedial method in question are likely to be met generally with scepticism, and not seldom with ridicule. Indeed, I have been frequently advised to abstain from showing the applicability of the method to the treatment of disease in general, and to concentrate my efforts in proving its efficacy as a remedy for one group only, namely, that of epilepsy and epileptoid affections. Were I to do this I should be unfaithful to my own convictions, and should leave to others the performance of a duty, which, for the sake of science and humanity, as well as my own self-respect, I am bound to discharge. When what is called a general law has been newly discovered, its operation and applicability become speedily recognised in a great variety of unexpected forms. This remark will, I venture to affirm, be found true with respect to the remedial principle in question.

**PART I. :**  
**DIARRHŒA.**





# DIARRHŒA.

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## CHAPTER I.

### DIARRHŒA ORIGINATED BY HEAT.

[In discussing diarrhœa I shall designate and arrange its different forms according to my conception of their causes—viz., as follows :

1. DIARRHŒA ORIGINATED BY HEAT.
2. DIARRHŒA ORIGINATED BY MOTION.
3. DIARRHŒA ORIGINATED BY (ELECTRICITY, OR ITS ANALOGUE) NERVOUS IRRITATION.
4. DIARRHŒA ORIGINATED BY THE MIND (EMOTION).
5. DIARRHŒA ORIGINATED BY FORMS OF FORCE, EITHER HITHERTO UNRECOGNISED AS CAUSES OF THE DISEASE, OR THE “MODUS OPERANDI” OF WHICH REMAINS UNKNOWN.

Having thus classed the various kinds of diarrhœa, I must observe emphatically that I do so only by way of recognition of the different aspects in which its cause presents itself; for I believe that, however diverse the forms which that cause assumes, it is always essentially one and the same. Heat; Motion; Electricity, or its analogue, generated by the irritation of the nerves of the alimentary canal; and Emotion (cerebral excitement), are correlative forces and fundamentally identical. When diarrhœa originates in a form, or forms, of force not seemingly identical with some one of the four just mentioned, it is probable that such cases of organic disturbance are nevertheless due to one of them, although it is as yet impossible to discover the chain of causal connexion between it and the disease in question. Meanwhile it is convenient to retain the fifth class here adopted in which can be ranged all those cases of diarrhœa originated by forms of force either hitherto unrecognised as causes of the disease, or the *modus agendi* of which remains unknown.

Scientifically arranged, the chapters on Diarrhœa originated by Heat, or Summer Diarrhœa, and that on Cholera, ought, according to my conception of the essential nature of those maladies, to stand in immediate relation, the former being at once followed by the latter. But there are some reasons why it is desirable as well as convenient to discuss the nature and treatment of the several forms of diarrhœa before proceeding to expound my views of what has

hitherto proved mysterious and inscrutable—the essential nature or proximate cause of cholera. One reason I will mention: the more intimately the reader becomes acquainted with the mode in which the diarrhœal flux is produced in each of the different kinds of diarrhœa, the more completely will his mind be prepared for the reception and consideration of my doctrine concerning cholera, the discussion of which, therefore, will form the concluding part of this book.]

Diarrhœa originated by heat comprises the ordinary Summer Diarrhœa of temperate climates, and the Premonitory Diarrhœa of Cholera, which, though not clearly traceable in a certain proportion of cases to their remote cause, are in the great majority of cases undoubtedly originated by heat.

“Diarrhœa,” says Dr. Farr, “is as constantly observed in English towns when the temperature rises above 60° F., as bronchitis and catarrh when the temperature falls below 32°.” After quoting this remark, Dr. Watson adds, “And there is a complaint—of which diarrhœa is one permanent symptom, but which is something more than mere diarrhœa—that shows itself in this country more or less every autumn, and prevails extensively in some years as a minor epidemic. It is rightly enough named *Cholera*, for it is attended with, and consists mainly of a remarkable flux of bile.”

*Summer diarrhœa* is, in fact, cholera in its first stage, and only needs a further rise of temperature or intensification of that state of the atmosphere productive of the disease to become developed into its characteristic and fatal form. Indeed, in those years when cholera prevails, diarrhœa is not only increasingly frequent, but is sometimes as fatal as cholera itself. “The largest total annual mortality from diarrhœa in England occurred in 1857, when the deaths from this cause numbered 21,189, the deaths from cholera being 1150. In 1854, when the mortality from the latter disease was 20,097, the deaths from diarrhœa amounted to 20,052.”\* Even as it shows itself in England during those summers in which no suspicion of the presence of cholera exists, it numbers many victims. In the course of some clinical remarks by Dr. Greenhow, at the Middlesex Hospital, reported in the *Medical Times and Gazette*, July 22, 1865, he said—“The mortality from this disease during the past week had been greater than during any corresponding week of the last twelve years, more than double that which took place during the corresponding weeks of any one of the years 1857, 1858, and 1859, when diarrhœa also caused a very large mortality, and more than five times as great as that during the same week of either of the years 1853 and 1854, in which the last epidemic of cholera occurred.”

It was at the date at which this statement was published. harm caused by the recognition of the

of, July 22, 1865.

presence of "Asiatic" cholera in England was sounded by the *Lancet* as follows: "Three weeks ago a patient was admitted into Guy's Hospital suffering from the characteristic symptoms of epidemic cholera. This case did not terminate fatally. On the 28th ult. a fatal case of cholera occurred in the Borough, which presented all the features of an aggravated form of the epidemic malady. The daily papers report a fatal case of 'Asiatic Cholera' at Newcastle, after twelve hours' duration."

If the symptoms in every case of summer diarrhœa were carefully watched, instances would not seldom be found in which its indubitable kinship with cholera would be easily recognised. Indeed, few English summers pass in which cases of clearly attested cholera do not occur,—cases developed in different districts remote from each other, at times when what is called "Epidemic Cholera" does not exist either in England or in any adjacent countries, and when, therefore, the most enthusiastic "contagionist," or advocate of a "cholera miasm," is troubled to account for the appearance of the disease. Dr. Greenhow states that "in the cases of diarrhœa which he had lately seen, and especially in those among children, the purging had for the most part been profuse, watery, and attended by much prostration. The pallor and anæmia which ensued, even after a brief attack, were indeed very remarkable. He had seen no case as yet this season to which the term malignant or Asiatic cholera could properly be applied; but in a few instances there had been vomiting and cramps, and the cases resembled what was called by Sydenham cholera morbus, and has since been termed summer cholera."

Indeed, already in the first week of June "sporadic" cholera was recognised, and the mortality from its after development, as well as from diarrhœa, in the metropolis was thus reported:—

1865. Week ending—	Diarrhœa.	Sporadic Cholera.	Mean temperature.
June 3	38	1	58.3°
" 10	44	1	63.6
" 17	93	3	57.8
" 24	187	11	61.1
July 1	184	11	59.5
" 8	301	12	66.1
" 15	267	11	61.4
" 22	280	18	63.6
" 29	252	23	66
Aug. 5	207	19	55.9
" 12	201	11	62
" 19	116	12	62
Carried over	2170	133	

## DIARRHOEA.

	Diarrhoea.	Sporadic Cholera.	Mean temperature.
Brought over	2170	133	
1865. Week ending—			
Aug. 26	115	6	61.7
Sept. 2	89	4	61.4
„ 9	89	9	67.4
„ 16	85	4	67.8
„ 23	92	3	60.9
„ 30	92	4	59.3
Oct. 7	83	5	56.5
„ 14	54	2	55.3
„ 21	62	4	46.8
„ 28	48	4	46.9
Nov. 4	61	2	42.5
„ 11	37	2	42.4
„ 18	32	None.	44.8
„ 25	28	None.	50.6
	3137	182	

Though it was generally known that during the autumn of 1865 cholera destroyed many lives at Southampton and several at Epping, very few persons supposed it present in London. Indeed, the metropolis was undoubtedly believed by the great majority of the people to be as free from cholera as it usually is, and yet during the five months from June to November inclusive 182 persons are reported to have died of that disease. It may be that some of these cases reported as cholera would have been described by other physicians as diarrhoea, choleraic diarrhoea, choleric, summer cholera, or British cholera; but this difficulty of discriminating between these various forms of diarrhoea and of cholera, and of distinguishing one group from the other, constitutes one of the strongest proofs that they are all one and the same disease, their differences of aspect being merely differences in the degrees of its intensity. Moreover, there can be no doubt that just as many of the cases actually reported as cholera would have been described by other reporters as diarrhoea, so many of those actually reported as diarrhoea would have been described by other medical men as cholera. As observed by the Registrar-General when commenting on the mortality from cholera which occurred in the metropolis during the week ending September 2nd, 1865—“Cholera is not a new disease. It is described in the earliest medical writings; and cases occur sporadically every week in summer in every country of Europe and Asia. . . . This form of cholera, called here English, or, more correctly, summer cholera, when fatal, differs little in appearance from epidemic cholera, which

is, however, well characterized by the short duration of fatal cases; by its attacking and destroying great numbers in its progress." In May, 1865, a man was admitted into Guy's Hospital suffering from what is described in the report of it as "Asiatic cholera," and the writer of the report remarks—"In calling this a case of Asiatic cholera, we simply imply that it was such a case as would have been so called during the prevailing epidemic of that disease. Dr. Wilks [who treated the case] said that every year he saw one or two such cases, but seldom so early in the year as this."\*

On the other hand, it is well known that in cholera-countries, when cholera is rife, many cases of diarrhœa which subside, instead of developing to the full proportions of cholera, present themselves,—cases in which the patients recover when no medical aid has been used, and which therefore cannot be regarded as cholera cut short by art, but which are, in fact, identical in all respects with the diarrhœa which prevails more or less, according to the rise and fall of temperature, in summer throughout Europe.

While I write, the same phenomena are repeating themselves in London. From July 18th to August 30th, 10 a.m., inclusive, the London Hospital admitted into its wards 495 cases of cholera, and 184 cases of choleraic diarrhœa; and simultaneously 9,494 cases of diarrhœa were treated as out-patients of the same institution.

That summer diarrhœa, cholérine, summer cholera, English cholera, sporadic cholera, and "Asiatic" cholera are phrases denoting various degrees only of one and the same malady, is strikingly manifest in the futile attempt of Dr. Copland (whose *forte* is the classification of diseases into genera and species) to point out the boundary line between diarrhœa and cholera. Under the head of *Diagnosis* he says:—"Diarrhœa differs from *cholera* in the much less severity of attack; by the absence of spasms of the extremities; by the entire absence, or occasional occurrence merely, of nausea or vomiting; and by the milder character and less rapid progress of the former. Bilious diarrhœa, however, is sometimes merely a slighter form of cholera; the existence of spasms in the latter constituting the chief difference, excepting as to grade; and *pestilential* cholera very frequently commences in some one of the common forms of diarrhœa."

Assuming the pathology of summer diarrhœa and the diarrhœa premonitory of cholera to be identical, an exposition of the symptoms and proximate cause of the one will of course serve for that of the other.

*Symptoms.*—Summer, or choleraic diarrhœa, is generally painless, and when preluding cholera is often the chief symptom observable during the few days immediately before the more fearful aspects of that disease present themselves. Hence it is that the onset of cholera, in a large number of cases, is peculiarly insidious,

\* *Medical Times and Gazette*, June 3rd, 1865.

the patient very naturally supposing that some irritating material in the bowel is causing slight flux, which being without pain will probably soon subside, and calls for no especial medical attention. As the malady progresses, and in some cases before any flux occurs, the patient experiences a sense of general lassitude or of mental and muscular weakness; the pulse, though often rapid, becomes feeble; the forehead, cheeks, and upper extremities become notably cooler than in health; the countenance is pale, more or less pinched and hollow; the eyes are often perceptibly sunken, and surrounded by a dusky or dark colour of the eyelids, especially the lower one. The abdomen is often cooler than normal, its lowered temperature contrasting strikingly with the warmer walls of the thorax. The patient not infrequently complains of a very disagreeable sinking sensation in the abdomen, and feeling of coldness there, which causes sufferers to resort to the use of hot stomachic stimulants, and which in Sweden, where diarrhœa is peculiarly prevalent, has led the peasants to adopt the plan of wearing, blacksmith-like, long leather aprons, which cover the breast and reach to the knees.\* The lower extremities are generally cooler than the upper, the feet being in many cases quite cold. When the disease increases in severity, cramps and vomiting are superadded to the above-named symptoms, and then a further intensification of the malady may merge it in cholera, distinguished according to its degree of severity by some epithet, as Choleraic Diarrhœa, Choleric, Sporadic Cholera, British Cholera, European Cholera, or Asiatic Cholera, conformably to the theory or the fancy of the medical attendant.

*Proximate Cause.*—It is commonly believed that summer diarrhœa is mainly caused by the eating of fruit, which is, of course, much more abundant in summer than winter. It is probable that in numerous cases this is an *exciting* cause of the malady; but I venture to assert that if this exciting cause were equally operative

\* "The principal sicknesses here are low fevers, colds, agues, and diarrhœa. Diarrhœa is very prevalent. The best cure I know is a handful of peppercorns, washed down with a glass of brandy. The peasants here adopt an excellent plan. They all wear long leather aprons, like blacksmiths, which cover the breast and reach to the knees. The great thing is to keep the stomach warm in winter, and I would always recommend the stranger who is much exposed to the weather here, to wrap a shawl or flannel round his body under his clothes."—*Ten Years in Sweden*. By "AN OLD BUSHMAN." London: 1865.

Some readers of this note may remark, with much show of reason, that if diarrhœa be so prevalent as stated in Sweden—a decidedly cold country—the fact does not confirm, but is opposed to my hypothesis as to the proximate cause of diarrhœa. This, however, is not the case. Besides diarrhœa, the principal sicknesses in Sweden are said to be low fevers, colds, and agues. Now all these maladies involve great disturbance of the vaso-motor nerve centres, but ague during the cold stage consists in their decided congestion; and when once this is established, whether by heat or other causes, a predisposition to diarrhœa is also established in the manner I contend for. Hence ague and diarrhœa are likely to be indeed, to attack alternately the same patient.

in winter as it is in summer, it would be very far from equally *efficient*. Moreover, thousands of persons who abstain from fruit, and who are wholly incapable of accusing themselves of committing irregularities of any kind in respect to diet, are, nevertheless, attacked with summer diarrhœa. They are at a loss to account for their sufferings; and hence ascribe them either to the influence of some mysterious, inscrutable agency, or (and indeed very commonly) to the action of the great heat in summer in some way unknown. If the diarrhœa were really caused by irritating ingesta, it is reasonable to suppose that the reflex action induced by them would result in sensations, more or less pronounced, of the nature of griping and colic: the intestinal contractions would be most likely to be not only especially vigorous, but concentrated both in time and intensity, and would therefore cause the bowel to pass along the offending material with undue force, thus originating the pains common in such cases. But in summer diarrhœa—if the hypothesis which I am about to introduce be correct—the order of causation is reversed: in the majority of cases, the dorsal nervous centres and those in intimate connexion with them which directly govern the alimentary canal, become suffused with blood much more copiously than is natural by the stimulant effects of the excessive external heat in hot climates, and of temperate climates in summer, and, perhaps, most rapidly by the direct rays of the sun on the back of the patient. The result upon the intestinal canal may be stated as follows:—

(a) The blood-vessels nourishing the tube receive a larger supply of nervous influence from the vaso-motor nerve centres than before, and hence, *contracting* more vigorously than natural, cut off to a proportionate extent the supply of blood to, and consequently the nourishment of, the intestinal walls. The bowels thus lose their wonted robustness, and so become, like a delicate lady with very little blood in her system, “highly nervous,” and susceptible of being excited and thrown into excessive or convulsive activity by a stimulus which in their healthy condition would but slightly affect them.

(b) I have stated in the introduction that in the first stage of bronchitis, when the bronchial mucous membrane is so swollen, from the abnormally large amount of blood circulating in it, as seriously to impede respiration, and when, at the same time, it is completely and painfully dry, the application of heat between the scapulae, by rendering the vaso-motor nerve centres more active than before, will not only stimulate the blood-vessels of the bronchial mucous membrane to contract, thus lessening the amount of blood circulating in the walls of the air-tubes and causing respiration to become much easier, but, simultaneously, will cause bronchial mucus to be freely secreted. Arguing analogically, I was led to the conclusion that when, by applying heat to the back, nausea is induced, the proximate cause of that nausea is the secretion of an abnormal



amount of mucus in the stomach; and I was led to the further conclusion that heat thus applied will also increase the secretion of mucus throughout the intestinal canal. Acting on this conviction, I have in certain cases of constipation indubitably accompanied, and, as I believe, caused, by a great want of intestinal mucus, overcome the constipation by the application of heat in the dorso-lumbar regions. Moreover, the nature of the innervation of glands generally renders it probable that the stage of spinal hyperæmia denoted by mere diarrhœa is associated with excessive secretion from the liver and pancreas, as well as from the mucous glands. Hence I believe myself justified in asserting that, in summer diarrhœa the flux is mainly constituted by an excessive secretion of bile, of the pancreatic juice, and of mucus, produced by excessive stimulation of the several glands consequent on hyperæmia of the spinal cord.

(c) Assuming the truth of the foregoing propositions, we see not only the source of a large portion of the choleraic flux, but also how the bowels have become in a condition rendering them peculiarly susceptible to that nervous influence which causes their peristaltic contractions, and thus the preternaturally frequent and rapid expulsion of their contents. Now, precisely those hyperæmic conditions of the nervous centres presiding over the bowels, which have resulted in the two groups of phenomena already explained, also induce an excessive exaltation of the excito-motor or reflex functions of those nervous centres, and thus cause them to transmit their stimulating influence to the muscular fibres surrounding the bowels with a copiousness and intensity far surpassing the normal amount. Hence these circular muscles, enfeebled, but rendered peculiarly excitable, as explained, contract far more rapidly and vigorously than usual.

The association of these three conditions—viz., enfeeblement of the muscular wall of the intestine, preternatural secretion from its mucous membrane, from the liver and the pancreas, and excessive muscular activity—all dependent, as I have shown them to be, upon hyperæmia of the nervous centres, constitutes the premonitory diarrhœa of cholera, and, indeed, all those choleraic forms of intestinal flux known as the summer diarrhœa of temperate climates. I may add, that while the hypothesis here propounded accounts for all the phenomena of the maladies in question, it receives striking confirmation, not only from the experience which I have already had in treating cases by means of ice along the spine, but also from the fact that I have induced diarrhœa by applying heat to the dorso-lumbar\* region.

\* I had under my care, in 1865, a lady suffering from long-protracted hæmorrhage from the bowels. The chief feature of my treatment, which was extraordinarily successful, consisted in the application of heat to the dorso-lumbar region, by means of the Spinal water-bag. After using the bag some weeks diarrhœa came on: it ceased when the bag was omitted, and recurred so decidedly each time when it was used again, that I was obliged to suspend its use altogether for a time.

If the proximate cause of the diarrhœa now in question be what I allege it to be, the hyperæmic state of the vaso-motor nerve centres induced by the heat must effect a diminution in the amount of blood in the *peripheral* arteries—not only of the bowels, but of the whole body; for the contractile force exerted upon the arteries generally will lessen their diameters, and will thus lessen the blood-currents supplying the capillary vessels throughout the system. It follows, necessarily, that the pulse will become feeble; that, textural nutrition being lessened, the organic, muscular, and cerebral functions will be impaired, and a sense of weakness and lassitude will be induced; and that, as the vital chemistry in every part of the structure proceeds less rapidly than before, because the lessened blood-currents supply less chemical materials than before, the amount of heat evolved will be lessened, and consequently the temperature of the surface of the body will fall.

*Prognosis.*—The mode of termination of the disease, if allowed to run its course uninfluenced by medical art will chiefly depend on the condition of the atmosphere, and notably on its temperature. A fall of temperature will conduce to the decline of the disease, a rise to its augmentation. When that peculiar state of the atmosphere which is causing diarrhœa to be epidemic, and which is induced by a high temperature, prevails, every case of diarrhœa ought to be suspected as dangerous, and to be promptly treated accordingly; whereas, when the malady is sporadic, or occurring only here and there in isolated cases, the malignant atmospheric influence is generally too weak to prove fatal, and the majority of cases of summer diarrhœa will spontaneously recover.

*Treatment.*—If the hypothesis which I have advanced concerning the proximate cause of summer or choleraic diarrhœa be correct—viz., that that cause consists in hyperæmia, more or less excessive, of the spinal and sympathetic nervous centres—and especially of those which preside over the bowels, and notably, therefore, of the superior and inferior mesenteric plexuses, it follows logically that if we could exercise a sedative influence over those centres by lessening the amount of blood in them, we should be able to avert, control, or cure the disease. Considering the secluded position of the mesenteric plexuses, placed as they are within the abdominal cavity, it seems, at first sight, improbable that we should be able to influence them by means of external applications. As a matter of fact, however, it is possible to lessen the amount of blood in nervous segments along the back at a considerable distance above or below those particular segments over which ice is applied. It is, indeed, a physiological fact, so regular in its recurrence that it may be denominated a law, that when one nervous segment is rendered either anæmic or hyperæmic, the adjoining one will assume a like condition, though in a lesser degree, while those further removed will, in

proportion to their nearness, also participate in the state impressed on the one first affected. It is, therefore, easily conceivable that nervous centres placed in a direction inwards from the point where ice is applied, may be influenced indirectly in the same manner as are those dorsal segments above or below the point of application. Moreover, as there is good reason for believing that the spinal cord itself, as well as those dorsal segments of the sympathetic from which the splanchnic nerves are derived, is intimately related functionally to the abdominal viscera, a direct sedative influence is, if this be the case, capable of being exerted by the application of ice along appropriate segments of the spinal region, while that influence is propagated indirectly to the mesenteric plexuses; and thus the whole nervous centres involved being supplied with less blood than before, are thereby rendered incapable of continuing those preternatural actions on which the production and continuance of diarrhœa depend.

These considerations led me to treat summer diarrhœa by the application of the Spinal ice-bag along the lumbar and lower dorsal regions in all cases; and when the cerebral and thoracic conditions are such as to permit the exertion of a sedative influence along the cervical and upper dorsal regions, to avail myself of the great additional power which may be exercised by applying ice in each cell of the bag at once along the whole spine. The application, in cases where there are no special reasons to forbid it, should be continued until the diarrhœa is subdued. Afterwards the bag should be applied at frequent intervals until the patient's strength is quite recovered. In ordinary cases it may be applied, after the purging has quite ceased, every four hours during a day, then every six hours, and then as the patient progresses, it may be used successively three times, twice, and finally once daily, until at length its use is discontinued.

The several effects produced by the sedative influence of the ice applied along the spine in cases of summer or choleraic diarrhœa may be stated as follows:—

1. The vaso-motor nerves emerging from the sympathetic ganglia convey less powerful nervous currents to the blood-vessels nourishing the alimentary canal than they were receiving before the ice was applied. They therefore cease to contract as they did before with abnormal vigour, and hence allowing streams of blood to course through them in increased volumes, the intestinal tube obtains again its wonted full supply of nourishment, and so regaining its normal robustness is no longer præternaturally excitable, and thus capable of being thrown into excessive or convulsive activity as it was before, if an unduly strong nervous stimulus be transmitted to its muscular coat.

2. The nerves emerging from the cerebro-spinal system and distributed to the glands of the intestinal mucous membrane, to the liver and the pancreas, convey less energetic currents from their

source to the several organs whose functional activity they regulate; and hence the secretion of mucus, bile, and the pancreatic juice becomes restrained, and thus the elements which poured out in excess constitute the fluid portion of the diarrhœal discharges are reduced to their normal amount.

3. In like manner the motor nerves of the circular muscles of the intestines are disabled from stimulating those muscles as vigorously as before; consequently the vermicular action of the bowels on which their expulsive force depends is subdued. Having become stronger, because more vascular, they have lost their abnormal excitability, and at the same time the intensity of the stimulus transmitted to them being lessened, they become quiescent, and no longer expel their contents with præternatural rapidity.

4. Simultaneously with the foregoing changes, the vaso-motor nerves throughout the whole body are experiencing the sedative influence which is being applied along the spine; the whole arterial system is therefore influenced; the arteries generally, the muscular coats of which were in a state of slight spasm, become relaxed; blood flows through them again with its usual copiousness; the full normal activity of textural nutrition in each structure is resumed; the evolution of heat, consequent on the chemical changes constituting this activity, reaches its usual amount, and a healthy glow is again felt throughout the body; the muscles regain their natural strength, the brain its wonted energy, and the general health is restored.

When I speak of applying ice along the whole spine, I do not mean that the ice-bag should reach along the whole length of the back, but simply that it should be nearly co-extensive with the length of the spinal cord. It should reach from the second or third cervical to about the third lumbar vertebra, or from a point about an inch below the base of the skull to the lower part of the small or hollow of the back. It is indispensable that the cold be prevented from coming in contact with the base or back of the head, and also that it be prevented from extending downwards over the sacrum.

So many grievous mistakes in selecting and applying the spinal ice-bag have come to my knowledge, that it seems to me necessary to give the following explicit directions:—

*Directions for selecting a suitable Bag in each Case.*—The spinal ice-bags are made of the following lengths:—8, 10, 12, 14, 16, 18, 20, 22, 24, and 26 inches. They vary in breadth from two to four inches and a quarter, the shortest bag being the narrowest, and the longest the broadest. The smallest sizes, or those 8, 10, and 12 inches long, are, in respect both to length and breadth, suitable for children; those 14, 16, and 18 inches long for youths of both sexes; those 20 and 22 inches long for women; and those 24 and 26 inches long for men. Of course there are cases in which a tall woman may need a bag 24 inches long; while one 22 inches long will be suitable for a very short man. If a bag is procured of

suitable length, its breadth, being proportionate, will be suitable also.

If it be desired to apply ice to a portion only, say a third, of the spine, it is of the utmost importance that a bag of suitable width be used. Ice applied in one of the small-sized bags to the back of an adult would not produce an adequate effect; whereas, applied in one of the large-sized bags to the back of a child, it would probably, by its great lateral extension, exert a depressing and otherwise injurious influence.

When it is necessary, in cases of children and youths, to apply ice to a part only of the spine, a bag of suitable size for the patient in question, supposing ice were about to be applied to the whole spine, should be selected; then, if only a third of the spine, no matter which part of it is to be acted on, the upper cell of the bag should be filled with ice, and applied where intended, the remainder of the bag being allowed to hang loosely, or folded up behind the cell which is used; if two-thirds of the spine is to be acted on, the upper two cells of the bag should be filled with ice and applied to the appropriate part, the lower cell remaining empty and dependent or folded upwards behind the cells in use, in the same way as when only one cell is used.

In applying ice to a limited part of the adult spine, either a spine-bag of suitable size for an adult should be used in the manner just indicated, or a short bag, called the "Lumbar Ice-bag," consisting only of two cells, and designed especially for the application of ice to the lumbar region, may be conveniently adopted.

*The necessity of observing these proportions is, for physiological reasons, extremely important.*

*Directions for preparing and applying the Spinal Ice-bag.*—Put ice (broken into pieces about the size of a nut or a small walnut\*) into the opening of the bag, on the side nearest to the loops, until the contents of the lowest cell reach up to the bottom of the second cell; then put ice in the middle opening until it reaches up to the bottom of the third cell; and finally fill the top cell. Then close the bag with the clamp, *placing it on the thickened, band-like part around the mouth*,† and letting the screws be on the same side as the loops of the bag. When the clamp is properly applied, a very moderate pressure by its screws is needful to make the bag

\* This is most easily done by means of an "ice-breaker," supplied by ironmongers, and consisting of a tapered piece of steel sharp-pointed, and fixed in a handle.

† These words are printed in italics in the hope of emphasizing the injunction they contain as strongly as possible. Many persons apply the clamp beneath the band, where, as there are two layers of India-rubber less than there are above, it is necessary to screw it more tightly than is needful when the clamp is applied on the band, in order to make the bag water-tight: by thus wrongly applying the clamp, its undue pressure is liable to cut the bag, and so destroy it.

water-tight. All pressure beyond what is needful injures the bag. Care must be taken that each cell is not so filled as to cause it to become round, otherwise only a small portion of the bag will touch the back. It is also expedient that the contents of the several cells should only slightly overlap each other.

In applying the bag, place the smooth side of it in apposition with the back, the side bearing the loops being turned outwards. The bag may be kept in its position in various ways. If it is intended to be used for only a short time, or if the patient be in bed, the best plan is to lie upon it, taking care that it is exactly along the centre of the back. If the patient should wish to move about, pass the elastic band round the head, in order that it may hold the upper cell close to the back of the neck, and sustain the bag by fastening the clothes tightly outside of it; or, pass a long tape through the lower loop, carrying each half of this tape over the shoulders, crossing one over the other in front of the chest, carrying them backwards round the waist, in order to close the bag closely into the small of the back, and then, bringing them forward, tie them in front. The bag will thus be sustained without allowing its weight to depend from the head. In this case the patient may move or walk about without any discomfort meanwhile.

As ice contains a considerable amount of air, this air, as the ice melts, accumulates at the top of the bag, and being a bad conductor of heat, prevents the still unmelted ice which it surrounds from exerting its intended influence; it is therefore necessary to unscrew the clamp occasionally, in order to let the air escape; and if the bag is to be worn for a considerable time, to replace the air by a little fresh ice.

*The Ice-bag Jacket.*—The methods just indicated of keeping the spinal ice-bag in its place will often prove sufficient, especially for adults, who when suffering only from diarrhœa, will themselves be able to co-operate with the nurse or attendant in taking care to retain the bag along the centre of the spine. But for children, and indeed in all severe and prolonged cases, much the most convenient and secure way of applying the bag, is by means of a jacket extending down to the hips, and fitting next to and close to the body. As it is extremely desirable in cases of summer diarrhœa and cholera, as well as of cholera, to make and keep the surface of the body warm, I advise that this jacket be made of flannel, which must be as unelastic as possible. It should have long sleeves extensible over the hands, and should be tied in front by means of several pairs of strings, so that by drawing them sufficiently, it can be made to fit tightly round the body. The better the jacket fits about the shoulders and upper parts of the arms, the better for the purpose in view. A jacket thus prepared must then have its central seam along the back which corresponds to the line of the spinal column cut open, from the line of attachment of the collar

down to the lower part of the hollow of the back. The seam uniting the collar to the back of the jacket must be undone on each side from the centre to an extent equal to half the width of the ice-bag about to be used. Then a strip of *thin linen*, the length and width of the ice-bag, must be attached to the inside of the jacket along the centre of the back, and must be securely sewn to the jacket along each side. In this way a sort of pouch to hold the ice-bag will have been made openable at the outside when the jacket is on the patient. The flannel which originally formed the central part of the back of the jacket, and which has been cut open along the central seam, now constitutes two flaps, one on each side: these must be hemmed and have corresponding strings attached to them at suitable distances. After the ice-bag jacket thus prepared has been put on, the ice-bag has only to be placed along the centre of the back of the jacket, over the strip of linen now forming the central part, and then the lateral flaps must be turned over it, and firmly tied together in several places along the central line. By this arrangement the bag may be kept most securely in its place, and may be taken away or replaced *with the least possible disturbance of the patient*—a very important consideration in severe cases.

In ordinary cases of summer diarrhœa the treatment now recommended will suffice to restore the patient to health; but if the temperature of the body has fallen considerably, the action of the ice-bag must be facilitated by using the utmost care to keep him warm. In all cases rest in the recumbent position should be enjoined; and if the patient is kept in bed covered with an abundance of clothing, so much the better. Patients who do not remain in bed must be particularly well clothed, especially about the extremities. Children should have lambswool armlets and leggings, and if they continue cold, vigorous friction should be applied to them. If notwithstanding these measures, along with the use of the ice-bag, the surface of the body should remain decidedly cool, then heat must be persistently applied to the extremities, and if the abdomen is cold, to it also. India-rubber bags containing hot water—*not unpleasantly hot*—are the best means of applying heat to the limbs. If these are not to be had, linen or cotton bags containing hot sand or salt, and ordinary stone or even glass bottles containing hot water, may be resorted to.

The effects of the spinal ice-bag cannot be too carefully looked for and watched, in order that if necessary its mode of application may be modified according to the changing conditions of the patient. If the head becomes hot, and the upper extremities warm, even while purging still persists, the ice must be removed from the upper third, or the upper half of the spine, and the application of the bag must be continued, and the mode of using it determined only by an appreciative interpretation of the significance of the symptoms from time to time observable.

Such is the treatment on which I rely for the cure of summer diarrhœa; and I have had abundant proofs of its efficacy without the aid of medicine. But if called upon to treat the malady without being able to resort to the use of ice, I should prescribe a mixture of sulphuric acid and quinine, as I have often found it quite efficacious. When I was last at Southampton, Mr. Bencraft, acting on my suggestion, tried this mixture extensively, and became convinced by experience of its peculiar efficacy. The dose I advise, for an adult, consists of half a grain of quinine, and seven or ten minims of dilute sulphuric acid, to be taken two or three times a day, or oftener if the symptoms be urgent. Concerning the various medicines most recommended or relied upon in the treatment of the disease, I cannot do better than avail myself of the following summary statement published in the *Medical Times and Gazette*, July 28, 1866:—

*“ Remedies for Diarrhœa. ”*

“ We proceed, as we promised, to give a comparative view of the remedies for bowel disorder. In so doing we go on the positive, not on the negative plan. That is to say, if practitioners of experience report that any given class of remedies proves efficacious in their hands, we accept the fact; and if other practitioners report good results from an opposite class, or apparently opposite class, of remedies, we accept that fact also. And we take liberty to use the solution of this seeming paradox which common sense and experience furnish us with. That is to say, we assume what we know to be true, that the cases in which opposite remedies are respectively used belong to opposite classes of cases, or else that divers remedies may attain the same end in the same class of cases by opposite routes.

“ The remedies which we propose to consider—shortly and in the space of one essay—we enumerate in alphabetical order.

Acids,  
Alkalis and absorbents,  
Antiseptics,  
Astringents,  
Calomel,  
Meat,  
Opiates,  
Purgatives,  
Stimulants.

“ The questions we have in view are these:—Supposing a man has to treat a crowd of patients affected with diarrhœa, what is the best general, shall we say routine, remedy? What, if used indiscriminately, will do good in the greatest number, and harm in the least number of cases? And, on the other hand, supposing time and circumstances allow of a discriminate and individual application of remedies, what are the cases for which each remedy is best adapted?



"We must also say, that we are discussing remedies for diarrhœa, and possibly for cholérine (this one word *cholérine* is better than *choleraic diarrhœa*), not for cholera, *i.e.*, the stage of collapse; further that the absolute repose and warmth of bed is a means of safety which it is difficult to over-estimate; and besides, as Dr. Drutt said, in his account of a visit to Amiens, it seems possible that a patient who is *cured* of diarrhœa shall yet die of cholera.

"1. The acids are the more worthy of trial by those who have never tried them, inasmuch as they seem at first to run counter to all our English opinions and traditions. Yet lemon-juice and salt is the old and popular remedy in Italy and the East for diarrhœa. Traces also of treatment by acids are found in the writings of some practical physicians of the last century. But, so far as we know, the person who first brought them into vogue in England was an obscure practitioner named Coleby, in Bermondsey, who gained a reputation and a fortune by his use of dilute nitric acid in large doses in the premonitory diarrhœa during the cholera epidemic of 1832. Since that time, the sulphuric acid has come into vogue, and its use has been advocated by many practitioners, as Mr. Boddington and Mr. Tucker, and it has now become so popular that it cannot be considered as the specialty of any one. Any acid appears to answer—sulphuric, nitric, phosphoric, hydrochloric, tartaric, or citric. Mr. Tucker pointed out, in 1854, the value of *cider*, with its malic, acetic, and tannic acids; but the sulphuric seems really the best and the most convenient. The right way of using an acid for diarrhœa is not to give it in any measured dose, but to cause it to be mixed with water, sweetened and flavoured with ginger—say half a drachm of dilute sulphuric acid, two drachms of syrup, and half a tumbler of cold water. The patient may dilute it and sweeten it to the precise point he likes, and may be told to drink it freely in as great a quantity as he likes; but few care for more than three drachms in the twenty-four hours. The effect of the remedy seems direct and positive. It stops diarrhœa, quells pain, puts an end to the generation of flatulence, and leaves the patient well. Most positively there is nothing purgative in its action. It is suited for atonic diarrhœa, with clean tongue, in every stage. Where diarrhœa has been going on, leaving the belly puffy with griping pains and flatulence, the acid acts like a charm. Again, in the cases of diarrhœa which affect overfed people—with foul tongue, yellow eye, and loaded liver (as we assume)—the acid is a good remedy by itself, but better if employed as an adjunct to a dose of calomel. Moreover, as a prophylactic, as Dr. MacCormac has shown, it is of very great value. The minute that a man says he can't eat through the heat, and lives upon beer, he should be advised to take some mineral acid in bitter infusion. It will not be taken for granted that there are no cases in which it fails. In some it fails, apparently without reason; and in cases where there is a red, irritable tongue, it should not be tried. But there is one almost certain indication—that if the patient likes it, it is almost sure to

cure him; if he dislikes it, it will almost surely do him no good. The acids, moreover, are adapted for all ages and conditions, from the infant at the breast with chronic, or atonic, or serous diarrhœa, to the veteran in his second childhood. The elixir of vitriol, or aromatic sulphuric acid, is nicer, and well adapted for private practice; but the common pure acid is quite good enough for most cases.

"2. There are the alkalis and alkaline earths, as chalk, to which, we suppose, bismuth must be added. There is no doubt that these remedies have a certain force, though not much in severe cases. They deodorize, or neutralize, or alter the quality of the intestinal contents and the intestinal mucous membrane, and (if employed alone) are suitable for infants and persons with red tongues and irritable, possibly ulcerated, bowels. The dose of chalk or bismuth should be very much larger than is commonly given, say 30 grains every hour—quite enough to produce a decided chemical change in the contents of the alimentary canal, and to be detected in the fæces.

"3. Antiseptics—*e.g.*, creosote, in doses of one drop in pills, is good in many an atonic diarrhœa. Its *modus operandi* seems clear.

"4. Astringents. Under this head we have the vegetable astringents, as gallic acid, catechu, tormentil, &c., adapted for atonic diarrhœa with clean tongue. But it must be observed that these remedies are seldom given without opium, and that they do not appear superior to sulphuric acid—certainly not so good for popular use. Of mineral astringents, the acetate of lead, in two or three large doses, is in most vogue, especially with Indian practitioners.

"5. Calomel is the English remedy *par excellence* for all cases attended with foul loaded tongue, yellow eye, thirst, headache, &c. We believe that it really has the power of checking pale serous stools, and of producing thick, offensive, coloured ones, and in the right cases is without doubt the most powerful and certain remedy we have. In the opposite class it is equally mischievous.

"6. Raw meat pounded into a pulp is a remedy which we first saw used by Professor Trousseau, at the Hôpital des Enfants Malades, in Paris, nearly twenty years ago. It is especially adapted for young infants, who take it greedily; it appears to be digested without effort, and, by giving healthy material to the blood, cuts off those causes of diarrhœa which consist in an impoverished state of blood. Its specialty is as a remedy for infants and children with whom nothing else agrees or is digested. But it is also the food *par excellence* for any case whatever, at any age in which food can be taken. Raw mutton is the thing, deprived of skin and fat, and pounded in a pestle and mortar till it is a mere paste. This may be given with or without a very little sugar to infants, in the dose of a teaspoonful at a time; adults may take a larger quantity diffused through cold beef-tea. We have established the fact to our own satisfaction that meat *raw* differs from meat cooked in being more digestible, although meat cooked, if pounded to a powder, and used to thicken broth, is a most useful food for adults, so soon as food is permissible.

"7. *Opium*.—That this blessed and priceless antidote to human woe should be useful in diarrhœa, is no more than might have been expected from its power of allaying irritation and pain and local afflux of blood. And the largest experience shows that with an ill-fed, ill-conditioned, below-par population small doses of opium act like a charm in a large percentage of cases of atonic diarrhœa with a clean tongue. Its action and use in such cases are consistent with the whole science and practice of medicine and surgery. Let us suppose that the *bowels are loaded*, and there are diarrhœa, pain, and spasm. The bowels are clearly unequal to the task of efficiently and vigorously discharging their contents; and in giving opium the practitioner does what the surgeon does when a patient with irritated bladder cannot pass his urine, and what the accoucheur does when a woman is plagued with inefficient uterine action. Opium rests a weary colon, and allows it time to gather strength to discharge its duties with vigour. There are two classical works on surgery — Vincent's "Surgical Observations," and Hilton's "Lectures on the Principle of Rest;" both concur in advising the use of measures to allay fretfulness of the whole body and of parts, and to give time. Any one who denies the value of opium in atonic painful diarrhœa, must be put out of the category of arguable persons. Equally ought that man to be sent to Coventry who affirms that it is a universal remedy. Opium used indiscriminately in cases in which acids or calomel or purgatives ought to be used—that is, in cases where the tongue is coated and foul, and there are evidences of loaded liver or *embarras gastrique*—may make the patient ill for weeks. We believe that we have seen fatal results follow the summary suppression by opium, of a salutary diarrhœa, in over-fed, gormandizing, red-faced, tight-bellied personages, especially women.

"8. *Purgatives*.—When diarrhœa occurs to persons who have previously suffered from constipation, in whom there are evidences of loaded bowels—fœtid, lumpy, quasi-dysenteric stools—the belly showing by palpation and percussion signs of accumulation in the colon, and the mouth and tongue nauseous, then the practitioner need seldom refuse to venture on a dose of rhubarb and polychrest salt or castor oil, preceded or not by a dose of calomel. When the motions are inodorous, and the belly is empty, the use of purgatives is simply cruel.

"9. *Stimulants*, such as ether, essence of camphor, the essential oils, as peppermint, Cayenne pepper, and more especially the almost too familiar prescription of a hot and strong glass of brandy-and-water, are of well-known efficacy in almost all cases of diarrhœa, except those attended with marked *embarras gastrique*.

"If we may sum up our own impressions of *single* remedies, we give the first place to the acids, as being more serviceable and less noxious than any other in the greatest number of cases; but for cases attended with well-marked symptoms of pain they are inferior



to opium, in those with *embarras gastrique* to calomel, and in those with faecal accumulation to calomel, or rhubarb with polychrest salt. Still they are the best single or universal remedy.

"Whilst the science of therapeutics demands the employment of remedies singly, the necessities of practice compel combination, in order to secure different or even opposite effects, and so to hasten the patient's cure. The acids may be used as auxiliaries to any other remedy if the patient relish them. Opium may be combined with stimulants in atonic painful diarrhœa, and the popularity of the compound called chlorodyne shows it to be a useful model for imitation now, just as itself is an imitation of the ancient cordials and theriacas. Opium, in fact, which soothes pain, may be combined with eliminative remedies, so as to add the *tuto* and *jucunde* to the *cito*. Of such combinations, the time-honoured one of calomel two grains, opium half-grain, is one which scarcely needs our praise. For a severe case it probably is the best thing that can be prescribed. It may also be combined with acids, or with alkalis, absorbents, astringents, and stimulants, as in the well-known compound chalk powder with opium."

Medicines judiciously used will undoubtedly, according to the general testimony of medical men, arrest diarrhœa in a large proportion of cases. I fear, however, that the certainty with which their efficacy in this disease may be relied on is greatly over-stated, and that each and all the drugs, chlorodyne included, which are extolled as specifics, are often resorted to in vain. Dr. Druitt in his interesting and instructive report of "A Visit to Amiens to see the Cholera," after observing that "in the right cases the old treatment of trisnitrate of bismuth and opium in large doses is the sheet-anchor," says,—“Of course it is not to be pretended that the diarrhœa could be checked in every case, nor that even if checked, cholera might not follow. They told me of the case of M. Thouiller, whose diarrhœa went on in spite of all remedies, so that (to use their own graphic expression) ‘*il cholérisa entre les mains des médecins.*’ Other like cases were told me. One there was too of a lady who had severe diarrhœa on Wednesday, was well apparently on Thursday, and *cholérisa* and died on Friday.” Sir Thomas Watson relates that in the autumn of 1854, he spent five weeks in a Welsh village where diarrhœa was exceedingly prevalent. He says:—“It yielded readily to scruple doses of the *pulvis cretæ compositus cum opio* of the pharmacopœia. The disorder frequently returned it is true, but that was owing, I believe, to some atmospheric condition which was in continuous operation.” I have no doubt that opium given “in large doses” with bismuth, as at Amiens, or as Sir Thomas Watson prescribed it, will for the time stop summer diarrhœa in a large majority of cases; but I have also no doubt that in many cases it increases the tendency of the malady to recur, and in an increasingly dangerous form. I shall

hereafter give my reasons for regarding opium as attended with danger when given in cases of cholera, and although I freely recognise the fact that many sufferers from summer diarrhœa get well after they have been treated with opium in some form, I regard its successes as temptations to be resisted, and carefully avoid its use in the treatment of this disease. In fact, if drugs may be confidently relied on to arrest summer diarrhœa, how is it that the disease, especially when it becomes epidemic, is so alarmingly fatal? As already stated, there were 20,052 deaths from diarrhœa in England in 1854, and 21,189 in 1857. In 1865, when during the six months from June to November inclusive, only 182 deaths from cholera are reported to have occurred in London; the metropolitan returns of fatal diarrhœa amount to 3137. During the eight weeks ending September 1, 1866, 4034 persons died of cholera, and 1787 of diarrhœa in London. It will surely be admitted that of the large number of persons destroyed by diarrhœa during the three years mentioned, at least, a considerable proportion had received medical aid, and had been treated by means of the various medicines usually resorted to in this disease with all the care and skill which the medical attendants would be sure to exercise. If so, the conclusion is inevitable, that summer diarrhœa is a malady which, in a considerable proportion of cases, defies the power of all drugs hitherto used to subdue it.

When all has been done that can be done by means of drugs, and the disease still persists, the physician, who then finally resorts to the spinal ice-bag and uses it, with heat to the extremities, &c. &c., simultaneously as recommended above, will, I believe, become convinced that at length he has got possession of a remedial agent which will induce in his patient's system precisely those restorative changes which hitherto he has been trying in vain to accomplish. If the patient has been previously dosed with opium or calomel, or other drugs calculated to impair seriously the health of the nervous system, the healthy response of the organism to the influence of the ice is not likely to be so rapid and striking as it would have been had he been treated from the beginning by means of ice. But in cases where large and frequent doses of opium have induced dangerously persistent hyperæmia of the spinal and sympathetic nervous systems, ice applied along the spine is, I believe, the only power capable of overcoming this condition.

*Prophylaxis.*—The preceding pages will have prepared the reader to expect me to propose the use of ice during periods of epidemic diarrhœa as a prophylactic agent; and, indeed, I freely avow my belief that were persons who are residing within the area of an atmospheric influence productive of diarrhœa to apply the spinal ice-bag during an hour, or, in many cases, even half an hour, once or twice daily, they would entirely escape the disease. And further I am of opinion that if, when the first symptoms of diarrhœa are manifested, the ice-bag be applied at once, the development of the



disease will be completely arrested. For medical men and nurses constantly moving in the choleraic atmosphere during the prevalence of epidemics of diarrhœa and cholera, the spinal ice-bag applied whenever they feel exhausted, "nervous," apprehensive of being themselves attacked, or when the bowels become in the least degree relaxed, will be found invaluable; for when once the refreshing and restorative influence of the ice has been thoroughly appreciated, not only is it resorted to for the arrest of any unpleasant symptoms, but it generates a feeling in the user of it that he possesses always at hand an effectual shield against any attack, and the confidence thus engendered is itself "a tower of strength." In this matter I speak, not only from my own experience, but from that of others.

As a prophylactic medicine, I recommend a grain of quinine in from 7 to 10 minims of dilute sulphuric acid twice a day.

#### CASES.

CASE 1. Mrs. J., aged about 60, suffering from diarrhœa, applied to me July 22nd, 1865, 11 A.M. Her bowels had been already moved that morning four or five times. She complained of feeling excessively "low," and of slight pain in the bowels. She was remarkably pale, and looked very ill.

I ordered ice to be applied in each cell of the Spinal Ice-bag, and to be continued until the symptoms should subside. Ice was applied forthwith, but only during one hour. From the time the bag was applied until 8 P.M. the bowels were never moved, and no pain was felt.

Meanwhile, she was extremely excited and troubled by hearing a piercing scream from a lady in the same house, who had been seriously hurt. At 8 P.M. the diarrhœa recurred, the bowels being moved again three times. At 9.30 the patient went to bed, and again applied the Spinal Ice-bag. She lay upon ice, and fell asleep; in about an hour she awoke, removed the bag, and from that time slept continuously till 6 o'clock the next morning.

July 23.—Between 7 and 11 A.M. the diarrhœa returned vehemently, the bowels being moved six or seven times; the arms and hands broke out into a cold sweat; the face was sunken; and the patient felt so ill as to be seriously alarmed, and to propose telegraphing for me from the country, whither I had gone on Sunday.

At 11 A.M. ice was again applied as before. During the first ten minutes the sense of cold which had been experienced during the morning, before the ice was applied, seemed to increase; but at the end of that time the patient suddenly became "hot." "I felt," she said (July 25th), "a glow of heat all over me, which has continued ever since." Meanwhile, she had no return of the diarrhœa, and, except that she had been weakened by the attacks, has felt quite well in all respects.

CASE 2. C. J., aged 44, complains, July 24th, 1865, that he has

not felt quite so well as usual during the last few days, and the bowels have been open more freely than usual. This morning he had a decided attack of diarrhoea. A three-celled Spinal Ice-bag was applied along the whole spine. At the time of its application he felt an urgent need of relief to the bowels by another motion; but the first effect of the ice was to cause that urgent feeling to subside. Only in half an hour afterwards were the bowels again moved; and from that time he did not experience another unpleasant symptom.

CASE 3. [The particulars of this case as well as of Cases 4 and 5 were supplied to me by Mr. David Mark Williams, Honorary Assistant Surgeon of the Liverpool Infirmary for Children.]

"July 12th, 1865, I was asked to see George B., aged 18 months, who, I was told, had been ailing for some time; he was now suffering from severe diarrhoea. On the second or third day of my attendance vomiting came on. Astringent mixtures, &c., were now given, with little or but temporary improvement; and by the 28th serious fears for the child's life were entertained by the parents.

"On the morning of this day I stopped all medicine, and ordered ice to the spine for half an hour. At 1 P.M. the parents had some confused idea that the cold had caused the child to faint; but I ordered the ice to be again applied for half an hour, the child being wrapped up in a shawl meanwhile.

"6 P.M.—While the ice was on, the child became quite warm, and free from pain, and in fifteen minutes was asleep, whereas it was crying bitterly before the application. It has not vomited since 3 P.M. Bowels still purged. The ice to be applied at 9 P.M.

"29th, 10 A.M.—After the ice was applied last night, the child fell into such a comfortable sleep that the parents would not disturb him, and the bag was left on until he awoke at 7 A.M. this morning. An hour afterwards the bowels were moved once—a tolerably firm stool. Nothing more was done; the child remained quite well when I left home, August 9th."

CASE 4.—"E. H., a girl thirteen months old (whose mother is an intimate friend of the mother of the child who is mentioned as Case 1), suffered from diarrhoea, was treated with ice twice a day and recovered. But as medicine was given and the gums were lanced at the same time, it is not so striking a case. Yet my opinion (and that of the parents) is that the ice greatly assisted us."

CASE 5.—"July 28th, 1865, J. J., male, aged 37, was attacked with Summer cholera. I saw him first on the 30th. He was then so bad that I should have considered the cholera Asiatic, but for the bile vomited. He had three doses of chlorodyne to relieve the cramps, and ice was applied to the spine. He was quickly relieved of the vomiting and purging; but the cramps were very trouble-

some on the 31st. He applied the ice three times, for an hour each time.

"August 1st. Cramps gone; skin warm, bowels not moved; no vomiting; but the patient continues thirsty. Nothing further was done, and he was quickly quite well."

[Though this case is rather one of cholera than of mere summer diarrhœa, I give it here with the two preceding cases, as the reports of them all were sent to me by Mr. Williams at the same time.]

CASE 6.—D. D., aged about 40, consulted me in August, 1866, when suffering from diarrhœa. He was not aware of having eaten anything likely to have brought it on. He was a good deal depressed, physically and mentally; and as he confessed to being involved at that time in a good deal of trouble, pecuniary and otherwise, it seemed to me likely that this had been the exciting cause of the malady—the predisposing cause being the high temperature and peculiar condition of the atmosphere at the time. At the date when I first prescribed for him he had been suffering more or less from diarrhœa during several days. Complete rest and energetic treatment at once, by means of the Spinal Ice-bag, was recommended to him; but he was unwilling to absent himself from business, and therefore only applied the Ice-bag at irregular and distant intervals. The diarrhœa was checked, and he expressed himself as generally better; but after returning home one evening, enjoying his supper as usual, and going to bed, he was woke up by a new and severe attack. Having only a small quantity of ice in the house, he could apply it but once before the following afternoon. At this time, when I saw him again, his bowels had been moved several times during the morning, his skin was cold and peculiarly clammy, his face looked pinched and sunken, and he was altogether much enfeebled. A fresh and abundant supply of ice having been procured, I ordered the Spine-bag to be applied continuously—the bag being replenished with ice each time the previous supply had melted—until the diarrhœa should cease, and meanwhile that heat should be applied to the extremities and to the abdomen, which was remarkably cold. Ice was applied about eight hours uninterruptedly, and afterwards he allowed a short interval to elapse between each bag: the purging became gradually less frequent, and by the following morning had wholly ceased. It was not until the ice had quite stopped the diarrhœa, which left him extremely weak, that he regained his natural warmth. He used the ice afterwards during a few days at increasingly wider intervals, and finally left it off, remaining quite well. No medicine was given.

CASE 7.—[I am indebted to Dr. J. Moorhead, of Weymouth, for the following report, extracted from a letter which he favoured me with Oct. 31, 1865.]—"Having recently suffered from a most severe attack of diarrhœa, for which I took largely the usual astringents



with opium, but without avail, I applied an Ice-bag to the lumbar region, which proved most grateful, and *immediately* arrested all the symptoms."

CASE 8.—James H. H., aged 18 months, who was brought to me July 28th, 1866, at 9 A.M., suffering from cholérine, has had a little diarrhoea "off and on" for a week past. At 8 o'clock last night he began to be violently purged, and at 3 o'clock this morning began to vomit. During the night he has been purged nine times, and has vomited four times. "The stuff that comes from his stomach and bowels," says his mother, "is like water and slime." He cried very much in the night and this morning, as if in pain, drew up his legs, seemingly cramped. The face is very pale and thin-looking, the eyes are rather sunken, and around the lower part of the orbits the skin is of dusky, or rather very light purple hue. The face and arms are remarkably cool; the legs and feet are cold. The child has suddenly become extremely weak. Pulse 132—distinct. I advised the application of the Spinal Ice-bag continually, and heat to the extremities, until the symptoms should subside.

3 P.M. Three bags of ice have been applied. In two or three minutes after the first one was put on, the child fell asleep, and slept a full hour. During the second and third applications he also dozed repeatedly. He has had two motions since the treatment began, more natural than before. He has been sick twice—once after taking milk-and-water, and once after taking arrowroot. He is generally warmer, and much more lively. Pulse 120. To put on a fresh bag of ice every two hours, and to keep the limbs well covered with woollen clothing.

July 29th, 10 A.M. Has had eight bags of ice applied since last visit, the last at 7 o'clock this morning. His mother says—"He sleeps every time the bag is put on: he seems to like it; he holds his head down to let the bag be put on directly I tell him the bag is coming, so I think it must be a comfort to him." Since yesterday he has had five motions; the last, which was at four this morning, was natural in colour, and of the consistence usual in ordinary diarrhoea. He has not vomited once; and has become much warmer. He slept altogether about four hours last night. Yesterday he ate some toast and arrowroot, and this morning had toast again." "He seems even fatter to-day," his mother says. He is now running about my consulting-room amusing himself. "Yesterday morning," as she remarks, "he couldn't have stood; he would have fallen down if I had tried to set him on his feet." To continue the ice throughout the day and night, applying a fresh bag every four hours.

July 30, 10 A.M. Bowels moved at 2 and 11 P.M. yesterday, and at 3, 7, and 8 o'clock this morning. Stools partly faecal, with very offensive smell. Vomited once, slightly, and was troubled with

wind and spasms about midnight. Slept or dozed as usual when ice was applied. His head and arms were hot all night, his belly and thighs cold. To apply ice along lower half of spine only, but continuously if the symptoms persist. To foment the abdomen if it is cold.

July 31, 8 P.M. Is altogether better. Had four bags of ice applied during the whole of yesterday, the last at 9 P.M. To-day three have been used—one at 7 A.M., one at 2, and another at 6 P.M. He slept during the whole time of application of two of the bags yesterday, and of the one last night, and continued to sleep till five this morning. He did not sleep (evidently not needing sleep) during the use of any of the bags to-day. His bowels were moved at 7 A.M. and at 2 and 6 P.M. to-day. The stool to-night was natural in all respects, except that it was of unusually light colour. This afternoon he was once very slightly sick; but he has eaten very well indeed to-day, and seemed to enjoy everything he has had. His thirst has abated. To-day he has been in capital spirits; has played about, and is evidently much stronger even than he was yesterday. He also looks much better. His mother says—"Throughout his illness he has shown signs at times of spasmodic pains," and that he has long been in the habit of having spasmodic jerks, or "a sort of tendency to convulsions." To apply two bags of ice during the night if he should be awake—not otherwise; and to-morrow to apply a bag every four hours.

August 1. Ice was applied at 11 o'clock last night: the child slept at once, and did not wake till 5 o'clock this morning. Ice was applied at 7 and 11 A.M., and at 3 P.M. to-day. He slept during the two first applications. Has not been sick at all; the bowels have been moved only once; they were not in the least relaxed; the stool was healthy. He is warm, in very good spirits, and has recovered his usual strength. To continue the ice two or three times a day.

August 9. The child has continued quite well.

The foregoing cases, though but few, exemplify and confirm the hypothesis which I have explained concerning the essential nature and proximate cause of summer or choleraic diarrhœa. If my theory and practice be proved after more extensive trial to be founded in truth (and I appeal to the reports of the cases of cholera, and especially of the unsuccessful ones, treated on the same principles, for further evidence that they are so), then the doctrine that the diarrhœal discharges are eliminative of a hypothetical organic poison, are conducive to the restoration of the patient to health, and ought to be encouraged—by doses of castor oil or calomel, for example—must be false, and the practice founded upon it must be generally dangerous, and probably not seldom fatal. If it be assumed for the sake of argument that the blood is poisoned, and that the only way of eliminating the poison is by the more or

less violent purging characteristic of the disease, it must be admitted that the great drain on the system which the purging effects, is, *per se*, a great evil, and only seemingly a good by comparison with the greater evil—death, which it is said to avert. But if the organic poison in question be purely hypothetical (and no *proof* whatever has been tendered of its existence), if, in fact, the blood is not poisoned at all, then every ounce of serum which is drained from it by summer diarrhœa is so much life lost—cast away without purchasing any corresponding advantage; and the loss is not only a great, but an unmixed evil which, if augmented by purgative medicines, is so at the peril of the patient's life; and if we prescribe them at the bidding of a theory, founded on no thoroughly ascertained facts, we incur a grave responsibility which should "give us pause."

If no proof has ever been adduced that the blood is poisoned, and if the drain on the system effected by the diarrhœa is an evil, we are not only not justified in giving purgative medicines, but are bound, on the other hand, to arrest that drain as promptly as possible, unless experience shows that there is no method of doing so which is not attended with an evil as great as the drain itself. Indeed, whether the blood be poisoned or not, experience shows that the safest treatment is that which stops the flux. Summer diarrhœa is becoming more generally recognised as the first stage of cholera, and the phenomena of cholera are generally held to be the results of a blood poison; but, as in the religious world men, and still more notably women, are often better than their creeds, so in the medical world the wisdom of theory is corrected and restrained by the more lowly wisdom of practice; and, excepting always that small chosen band of consistent theorists who meet adverse facts with the bold explanation—" *Tant pis pour les faits*," the great body of physicians to whom in hypothetical vision the diarrhœal discharge is revealed as a beautiful provision of nature for the elimination of the choleraic poison, concur in exerting their utmost efforts to stop that discharge as the surest means of averting the supervention of cholera itself. Thus from every side comes the admonition—"Stop the flux of summer diarrhœa; stop it by any method which proves effectual; but stop it at once and completely." I have already referred to the danger attendant on the use of opium. Whatever advantages may be claimed for calomel, certain it is that just in proportion as it produces its characteristic effects, it produces permanent constitutional evil. Chalk, the trisnitrate of bismuth, and the usual vegetable astringents, are, I believe, unobjectionable so far as their influence on the nervous system is concerned; but they are often rejected by the stomach, often fail of their purpose, and, however successful they sometimes prove, they are always very disagreeable to the patient. Sulphuric acid, which seems to me the least objectionable and most effective medicine for summer diarrhœa (especially when combined, as I

advise it to be, with quinine) is, however, open to three objections—*Firstly*, when there is a tendency to vomit it is often rejected by the stomach; *secondly*, it frequently fails to arrest the discharge; and *thirdly*, it produces in some patients painful astringent effects on the lungs—dragging sensations as if the pulmonary tissue were powerfully contracted or drawn together, and not infrequently in the same patients distressing feelings as of partial suffocation. Now it seems to me reasonable to infer from these facts, observable in peculiarly sensitive patients, that though sulphuric acid is a powerful astringent of the intestinal glands, and of the alimentary mucous membrane generally, its action becomes dangerous to patients whose diarrhœa is of that advanced stage when it is just merging into the algide phase of cholera; for then the vascular system is in a state of extreme constringency, and it seems difficult to avoid the conclusion that the sulphuric acid will but increase the existing evil. And though Dr. Wurms commends its use even in collapse, the experience of physicians in this country does not justify his recommendation. The spinal ice-bag is open to none of these objections, while it fulfils all the requirements of the case: the presence of vomiting is no objection to it, for it is not taken into the stomach, and it stops vomiting; whereas the objection to opium is that it congests the nervous centres, the ice-bag renders them anæmic; whereas an objection to sulphuric acid is that it constricts the lung-tissue and blood-vessels, the ice-bag relaxes the one and the other; whereas all the medicines passed in review often fail to arrest summer diarrhœa, I have never known the ice-bag to do so; and finally, whereas most of the said medicines are loathed or disliked by the patients, they generally look forward to the application of the ice-bag as a comfort and a pleasure. For a proof of this assertion I refer to the report of Case VIII. While I write, a note (dated July 31st, 1866) has just reached me from a physician who is making extensive use of spine-bags in Scotland; he says:—"One thing has struck me much since I wrote you last—namely, the liking that sensitive, chilly patients have for the cold bag to the spine, although frightened to think of it before they make trial." In 1863 Dr. Druitt came to my house to see the results of my treatment of paralysed and epileptic patients, of whom he saw five. He subsequently wrote me a letter, giving his impression of what he had seen. After confessing that he was agreeably surprised at the results of my treatment, and stating that "there was no mistaking the testimony of the patients that those results had been most beneficial," he observes—"I learned from all the patients that the treatment had made them 'more comfortable;' I mean as regards their general feelings of health and animal sensations, without reference to the relief of particular symptoms. In my own practice, I lay the greatest stress on this point. It is true that some methods of treatment are ultimately beneficial, although they may be most

repulsive or nauseous, and give great discomfort and *malaise* for the time ; but it will not be denied that, *ceteris paribus*, that treatment is most likely to do good which shall seem congenial to the whole feelings, whilst meanwhile the patient is saved much annoyance and misery."

## CHAPTER II.

### DIARRHŒA ORIGINATED BY MOTION.

THE most eminent physicists are now in the habit of speaking of heat as a mode of motion. The hypothesis already put forward affords a physiological illustration of this conception; and conversely, motion, in the human organism, may, for all practical purposes, be regarded as the equivalent of heat. The diarrhœa which frequently accompanies sea-sickness is an illustration of this truth. For an account of the pathology of sea-sickness, the reader is referred to my pamphlet on that subject; but with respect to the diarrhœa sometimes associated with it, I will state here how I believe it to be induced. The motion of the ship is communicated to the abdominal and pelvic viscera, which, rising and falling, coming in more or less violent concussion with each other and with the abdominal walls, and having their contents also violently moved to and fro, thus receive an enormous number of abnormal impressions: these are conveyed to the superior and inferior mesenteric nervous plexuses, which transmit the complaint to the vertebral ganglia of the sympathetic. They forward it along the *rami communicantes* to the spinal cord, which, if thus excited to a considerable degree, sends unusually energetic motor impulses in the reverse direction to the circular muscles along the complaining organs. Hence these muscles contract with excessive vigour, and, co-operating with the preternaturally active mucous membrane, and other secreting organs along the alimentary canal, induce diarrhœa. It will be seen that in this case motion originates irritation; irritation, propagated to the nervous centres through the sensory nerves, causes an unwonted afflux of blood in those centres; their functional power becomes thereby increased, and, consequently, their reflex actions become proportionately more numerous and intense. The conditions of the nervous centres in question thus induced by motion are closely analogous to those which in choleraic diarrhœa are induced by heat; and the results, so far as the bowels are concerned, though less in intensity, are the same in kind as those which heat produces. It is, I apprehend, probable that the reason why, in cases of sea-sickness, one person suffers from diarrhœa and another does not, is that in those who do thus suffer, the special segments of the nervous system, which preside over the nutrition and functions of the



bowels, are especially feeble and excitable, and therefore peculiarly liable to derangement.

But besides the motion of ships, there are other forms of motion which will originate diarrhœa. Sometimes when sickness is induced by travelling in a railway carriage, or other vehicle on land, the bowels become relaxed; but the most usual form of motion which on land originates diarrhœa is that of vigorous walking or running. Of course only in so-called "nervous" persons—*i.e.*, those in whom the spinal cord and sympathetic ganglia are in a state of chronic hyperæmia, or those in whom hyperæmia of those centres has been temporarily induced by the solar heat—does the remarkable phenomenon in question occur. Referring to a case of chronic diarrhœa, Sir Ranald Martin says,—“An officer whom I sent to Bourne-mouth missed his way home, and had to walk beyond his powers. The result was a relapse which nearly proved fatal.” A gentleman, aged 34, who consulted me in Nov. 1865, and who from boyhood had suffered extremely from a group of nervous symptoms, all referable to chronic congestion of the nervous centres just mentioned, had been for years the subject of what, at the risk of being laughed at, I venture to call *chronic cholera*. During several years he has suffered from “a terrible chilliness all over;” his hands are habitually dark-coloured and blue from want of circulation in them, and are remarkably cold; his extremities are extremely wasted, and his general aspect is that of a person wonderfully shrunken. He was always, he says, “a poor lean shadow.” He suffers from “almost utter absence of digestive power” and “general want of nervous energy.” Very little wine affects him uncomfortably, causing dryness of the skin and of the mouth, and *increases his habitual coldness*. Bowels opened regularly every morning immediately after breakfast, except after physical exercise, *when they become loose*. If he were to walk he would most likely have diarrhœa. In June, 1865, he went with his wife to Switzerland, and having while there walked four or five miles in one day, produced a diarrhœa which lasted three days. Previously he had been in the habit of walking, and of having diarrhœa afterwards. Since he has ceased to walk the diarrhœa has ceased.

It is curious to observe that in this case wine seems chiefly to have increased the hyperæmia of the sympathetic ganglia, thus causing contraction of the arteries generally, and therefore increased the general coldness of the body. In another case under my care these centres, as well as the spinal segments most directly related to the bowels, are peculiarly liable to have their blood-vessels suddenly dilated; for if the patient takes brandy-and-water he is sure to suffer from diarrhœa. Another patient under my care has her bowels powerfully acted on if she drinks a wineglassful of sherry at night.

2. Recurring to the doctrine that the motion of walking will induce

diarrhœa, I may mention here a further illustration of it. A gentleman, upwards of 30 years old, who consulted me July 30th, 1866, and who has long suffered from various troubles of the nervous system, associated with habitually cold feet and frequent coldness of the forehead, is at once attacked with diarrhœa if he takes a considerable walk, or if he has any considerable mental excitement. The mode of origin of the malady in this case illustrates not only the doctrine propounded in this chapter, but also that explained in Chapter IV.—viz., that in many cases, through the agency of the spinal cord and sympathetic ganglia, diarrhœa is originated by the mind.

*Treatment.*—The diarrhœa of sea-sickness and that originating in motion of any kind is essentially the same in principle as that of summer diarrhœa ; but for especial directions and illustrative cases, readers are referred to my pamphlet on "Sea-sickness."





### CHAPTER III.

#### DIARRHŒA ORIGINATED BY (ELECTRICITY OR ITS ANALOGUE) NERVOUS IRRITATION.

SUMMER diarrhœa, *pure and simple*, is, according to the theory I have advanced, of almost wholly centric origin ; but undoubtedly a considerable proportion of these cases would not be developed were it not that eccentric exciting causes give the *coup de grace* to the already abnormally hyperæmic nervous centres. The diarrhœa associated with sea-sickness has a simultaneous and two-fold origin: the exciting cause acts on the nervous centres directly, and indirectly through the agency of eccentric irritation, and thus of reflex action as already explained. Of course, in so far as diarrhœa is a result of eccentric irritation and reflex action, whether it be nominally summer diarrhœa or diarrhœa associated with sea-sickness, it should, strictly speaking, be classed under the heading of the present chapter. Diarrhœa arising from the irritation of dentition, from irritating ingesta, from drinking impure water, from inhaling noxious gases, from ulceration of the bowels (as in phthisis), and the symptomatic diarrhœa of numerous diseases, are forms of the malady in which its primary cause operates in the first instance on the mucous membrane of some part of the alimentary canal, the nerves of which are irritated: the nerve currents (probably electric) to the sympathetic nerve centres and to the spinal cord are greatly increased in number and intensity; these organs, being thus unduly excited, receive an extraordinary afflux of blood; their functional power is proportionately exalted; their reflex activity is intensified, and the phenomenon of intense reflexion along the alimentary tract—diarrhœa, often associated with nausea and vomiting, pallor, coldness of the surface, feeble pulse, weakness of all grades, and, in its extreme form, with fatal collapse—necessarily ensues. In all these instances, however diverse the *primary* cause of the disease, the *proximate* cause is always the same, and is identical with that of those forms of diarrhœa reviewed in Chapters I. and II.—viz., hyperæmia of the spinal and sympathetic nervous centres, including especially the mesenteric plexuses.

During periods of epidemic summer diarrhœa the great majority of its victims are children. The Registrar-General, in his weekly statement of July 30th, 1866, which shows that during the previous

week there had been 349 deaths from diarrhœa in the metropolis, observes—"The epidemic takes the form of diarrhœa chiefly in young children; thus 309 of the 349 deaths from diarrhœa last week were children under five years of age, including 244 infants." Of the 309 thus carried off a large proportion suffered, I doubt not, from teething, and a considerable number were probably troubled, in the first instance, by offending materials in the alimentary canal.

In the cases of dental irritation, exciting impressions are of course transmitted along the maxillary branches of the fifth nerve to the medulla oblongata; the irritation thus set up in this nervous centre extends downwards along the cord and laterally to the sympathetic ganglia, and from them is reflected on the abdominal viscera, producing diarrhœa—also very often vomiting; and not infrequently the reflected impressions are radiated simultaneously not only to the vascular system, thereby causing arterial spasms and a fall of the bodily temperature, but to the voluntary muscles, thus inducing cramps, muscular twitchings, and convulsions—the frequent precursors of death.

When irritating ingesta are the immediate exciting causes of the diarrhœa, the eccentric irritations along the intestinal tube propagated to the nervous centres are reflected back upon it through more contracted arcs, and more immediately than is the case when the source of the irritation is dental; inasmuch as the nervous centres upon which the impressions first impinge are the same as those which reflect impulses on the bowels.

Water containing organic matters undergoing decomposition is perhaps, of all the exciting causes of diarrhœa, at once the most general, the most subtle, and the most potent. Pure water, especially in summer, is rarely to be had, while a very large amount of that which is used for drinking purposes by most communities contains a varying proportion of organic matter, and in many instances a proportion so great that the wonder is those who drink it resist its baneful influence to the extent they do. Its poisonous operation is peculiarly subtle, because precisely when those chemical changes occurring in its contents are most active it is fullest of bubbles of carbonic acid gas, and is thus attractive at once both to the eye and to the taste—sparkling and grateful. If water of this kind be confined, as in a closed well, the sparkling character of draughts from it is very striking. Of course if, during the process of decomposition, a large surface of such impure water be exposed in summer, that process will be more rapid, the greater part of the gas will be evolved as it is formed, and the water will be perceptibly offensive. The great poisonous potency of such waters arises from the fact that as they hold the organic matters in solution, they are capable, when drunk, of bringing those matters in contact with every part of the alimentary mucous membrane, which being thus generally irritated, and having an enormous surface, converges and concentrates an amount of exciting influence upon the nervous centres greater than

can be produced in any other way except that of powerfully exciting the whole surface of the skin. This consideration fully explains how it comes to pass that of all the *exciting* sources of diarrhœa and cholera which can be at once pointed out and localized, bad water is the greatest and most indubitable. Of course when the eccentric irritation of the alimentary mucous membrane by bad water has been effected, the physiological processes by which that irritation is transmitted to the nervous centres, and by them transmuted into motor impulses, which become distributed over the mucous tract, and the voluntary and involuntary muscles, is identical with those previously described. Diarrhœa is thus an almost inevitable result, while the more grave developments of the same disease are scarcely likely to be far off.

Noxious effluvia probably stand next in importance to impure water as exciting causes of diarrhœa. The fact is familiar to all persons, that the sudden inhalation and still more the swallowing of such effluvia in a concentrated form suffices to cause nausea and even vomiting. Indeed, some persons are so sensitive in this respect that merely smelling such effluvia will make them sick. Now it is reasonable to suppose that if these violent effects are producible by effluvia in a concentrated form, the prolonged influence of the same, even when they are greatly diluted by atmospheric air, will produce effects which, being cumulative on the peripheral nerves of the mucous membranes influenced, will suffice to originate those reflex actions resulting in diarrhœa.

The diarrhœa associated with the advanced stages of phthisis, is an example of the most direct reflex action producing an intestinal flux of any with which I am acquainted. For, in this case, the irritation which arises in diseased intestinal glands is restricted to a small area, and is directly reflected as motor impulses on glands of the same structure and of the same function. And it is remarkable, and at the same time explicable by this consideration, that in these cases the diarrhœa is not accompanied in any marked degree by those various phenomena which constitute the additional characteristics of the typical summer diarrhœa mainly of centric origin.

Respecting those other examples of diarrhœa symptomatic of, or occurring in the course of, various diseases, no statement in detail is necessary; they are severally explicable on the general principles already enunciated, and the peculiar features and circumstances of each case will be easily appreciable by every intelligent physician who, having verified and accepted those principles, will readily modify and adapt the treatment which they dictate to the exigencies of each individual case.

Summing up and characterizing the causes of the several forms of diarrhœa briefly reviewed in this chapter, we may say that the *pre-disposing causes* consist in the high temperature, and, consequently, the disturbed electric conditions of the atmosphere in summer; that the *proximate cause* consists in the hyperæmic conditions of the

nervous centres which the predisposing causes have induced; and that the *exciting causes* consist in the several forms of eccentric irritation of the terminal branches of the sensory nerves already enumerated—viz., dental irritation, ordinary offending ingesta, bad water, noxious effluvia, and the various internal excitants incident to disease, of which the glandular ulcerations of the intestines occurring in the course of phthisis are typical examples.

*Treatment.*—The general principles of treatment described in Chapter I. are strictly applicable in all the cases just mentioned. In applying those principles, however, great discrimination is necessary, inasmuch as the precise method adopted must differ in different cases. When the diarrhœa arises from dental irritation, it is desirable, while applying ice along the spine, also to apply it across the base of the brain, by means of what I call an ice-pillow, in order to exercise the utmost possible sedative influence on the medulla oblongata, and meanwhile to restrain the tendency to cerebral congestion which frequently accompanies this form of irritation. Diarrhœa resulting from ordinary offending ingesta, bad water, noxious effluvia, &c., should be treated, so far as the use of ice is concerned, exactly in the same way as ordinary summer diarrhœa; but, obviously, where there is reason to believe that the intestines contain crude and indigestible or undigested substances, the long-established practice of giving some appropriate purgative medicine should be resorted to. In restraining, by means of ice, the diarrhœa incident to phthisis, peculiar care is necessary, lest hæmoptysis be induced: in these cases the ice can only be safely applied, in the first instance, in the lumbar region; its application between the scapulæ is, as a general rule, dangerous, and can only be safely made after the physician has satisfied himself that there is no probability of causing either pulmonary hæmorrhage or congestion. Only so long as the dorsal nervous centres still remain in a state of considerable hyperæmia, can the danger in question be said to be absent.



## CHAPTER IV.

### DIARRHŒA ORIGINATED BY THE MIND.

AMONG the most remarkable and decisive proofs that the *proximate* cause of diarrhœa has its seat in the nervous system, is the well attested and indeed often observed fact that the disease is inducible by mental emotion: mental shocks of various kinds (especially fright) frequently produce it. Sir Thomas Watson observes:—"A curious exciting cause is to be found in *mental emotions*, and especially the depressing passions—grief, and, above all, fear. A sudden panic will operate on the bowels of some persons as *surely* as a black dose, and much more *speedily*. Among the circumstances which *predispose* most persons to this kind of malady we may particularly specify *season*—the hot weather of summer and autumn."

The following facts I am able to authenticate:—

(1.) A woman who has a drunken husband suffers great anxiety when he is away from home, especially if late at night, lest anything should happen to him, and particularly lest by a fall or other accident he should receive bodily harm. This anxiety brings on diarrhœa, accompanied with trembling, pallor, and a peculiar haggardness of countenance.

(2.) A lady who, while crossing the Atlantic, suffered fearfully from sea-sickness and violent diarrhœa during the whole passage, has since her marriage been troubled with diarrhœa almost always when she has experienced painful emotions. On almost every occasion when her husband is unkind to her, as he is wont to be, she has a violent attack of diarrhœa.

(3.) A woman who suffers from diarrhœa whenever her feelings are vehemently excited, even although the excitement may be one of sudden pleasure.

(4.) One of my patients, who was reading George Eliot's noble work "*Romola*," assured me that the emotions it excited in her brought on diarrhœa! In fact, owing to this remarkable transformation of emotions into "*motions*," she was obliged for a time to abstain from reading the book.

(5.) A lady, one of my patients, when affected by any violent emotion, especially if of a distressing character, is almost immediately attacked with diarrhœa, or vomiting, or both.

(6.) A gentleman, one of my patients already mentioned, always finds himself attacked with diarrhœa after he has experienced any considerable mental excitement.

It is well known, moreover, that many soldiers, especially young ones, are attacked with diarrhœa when going into action.

In these cases congestion of the nervous centres appears, physically speaking, to be the primary fact. But I apprehend that the order of causation is as follows:—Terrifying or exciting impressions, suddenly communicated, are conveyed to the sensory ganglia, and are thence distributed to the cellular structure constituting the cerebral convolutions; these are thrown into tumultuous excitement, which is propagated along the motor tracts with the rapidity of lightning down the whole spinal axis, and laterally to the ganglia of the sympathetic; these becoming suddenly swollen with blood instantly act with intensely vehement energy, and diffuse their subtle stimulus in all directions. As the source of the powerful impressions which they have, in this case, received, is cerebral, so the *chief* direction in which the vaso-motor impulses are reflected is towards the brain: hence, quick as thought, the cerebral arteries are contracted with preternatural energy, and thus, in extreme cases, the brain being rendered comparatively bloodless, the person is stunned as if by a blow; the face becomes pallid, cold sweat sometimes exuding from it; if consciousness is not abolished, mental power is still greatly enfeebled; and, by the same agency and in the same manner as the phenomena of ordinary diarrhœa are induced, the strength of the whole muscular system is impaired, the temperature of the surface of the body is lowered, the muscular wall of the intestines is enfeebled, their mucous membrane exudes its appropriate secretion superabundantly, their peristaltic action becomes excessive, and thus diarrhœa originating in mental emotion is clearly due to the same *proximate cause* as that on which, as I have endeavoured to show, all forms of the malady depend.

*Treatment.*—Ice along the whole spine until the malady is completely subdued, unless the vascular conditions of the brain and lungs present indications to the contrary. Ice can never be safely used without the most careful and just appreciation of these conditions, and must be applied along the whole spine, or to some one part of it only according to the peculiar exigencies of each case.

In the course of my practice I have had numerous experiences of the wonderful efficacy of the spinal ice-bag in arresting various kinds of diarrhœa, but think it needless to load these pages with a great accumulation of illustrative cases. Perhaps of all the forms of diarrhœa that which is associated with a general and long protracted impairment of the nervous system is at once the most distressing and weakening, because the most continuous, and is the most amenable to ordinary medical treatment. I will therefore cite

an example of this kind, and that one of the last which has come before me.

CASE 9.—A. B., a gentleman upwards of 40 years old, complained to me, June 25th, 1866, of suffering constantly from slight diarrhœa, which has been of long continuance, and to which he has been peculiarly liable for several years past. This is associated with great "nervousness," which consists in a feeling of numbness at the back of the head, a kind of hysterical depression, weakness of the extremities, of the upper the most, and most of all of the left arm and hand. Though he has never lost the power of writing with the right hand, he has lost the power of writing steadily. Cannot carve the meat at dinner. His ailments so trouble him that he shrinks from going into society, avoids dining out, or going to evening parties. He has never slept well, but has slept somewhat better lately; he lies awake for long periods at a time, and if he ever sleeps heavily, he knows that he is in worse health than usual. Does not suffer from headache: but very frequently indeed, especially when he is exhausted, the head is extremely cold. Also when in this state a film sometimes comes over the eyes. The pupils are often exceedingly dilated at night. He is always troubled with some irritation of the bladder, often with slight involuntary micturition, and during the presence of diarrhœa the urine is pale, very abundant, and very often voided. The diarrhœa is also associated with pain in the lumbar region. Pulse 88, full and strong.

I recommended this patient to apply ice along the whole spine thirty minutes each morning and forty-five minutes each night, and to take as follows—*R* Quinæ disulphatis gr. j, acidi sulphurici diluti ℥x, ex aquâ, bis die.

I saw this patient again July 11th, or sixteen days afterwards, when he gave me the following report:—He felt the effect of the ice very strikingly during the first application, especially in making the lower extremities along their whole course "quite hot." He has used no medicine of the kind he has ordinarily resorted to with a view to restrain the diarrhœa, and nevertheless has not suffered from the malady at all. The peculiar feeling at the back of the head is almost wholly removed. The head has not been cold once; on one occasion only has the film come over his eyes; and he goes out to evening parties without hesitation. He has slept "wonderfully well" since he began the ice. The power and sensibility of the left hand have greatly improved; in evidence of the general improvement of the upper extremities, he says that he can write much more steadily than before, and that he has managed to carve up a fowl with becoming expertness. The urine is less abundant, and of a healthier colour. The slight involuntary micturition is almost wholly overcome. He remarks to-day that the fæces have a considerable amount of mucus in them; this has long been the case. Pulse 76.



There are four important facts observable in this case which bear directly as illustrative evidence of the truth of the doctrine which these pages advocate. *Firstly*, the head of the patient is frequently very cold, a film coming over the eyes at the same time, especially during exhaustion; this denotes hyperæmia of the sympathetic ganglia governing the cerebral arteries. *Secondly*, the urine is said to have been very abundant and pale; this denotes that the kidneys were unduly stimulated, and hence that the source of their stimulation, the lower segments of the spinal cord, were hyperæmic. *Thirdly*, while the diarrhœa existed there was pain in the lumbar region; this denoted the same condition. *Fourthly*, the fæces are said to have been covered with a considerable amount of mucus; this denotes excessive activity of the mucous glands, induced, again, by the hyperæmic spinal cord. Conformably with this interpretation of the phenomena in question, the production of comparative anæmia of the spinal and sympathetic nervous centres should, theoretically, be the surest way of curing the patient. Ice along the spine as applied is the most effective method of producing that anæmia: the patient's report of the results speaks for itself.

CASE 10.—In evidence of the truth of the doctrines expounded in this pamphlet, and of the efficacy of the curative method founded upon them, I gladly avail myself of the testimony of witnesses presumably more impartial than myself. On the 28th of June last I received from a physician a letter containing the following passage:—"Certainly your system gives me greatly increased power over the complaints which formerly were least under control. I have been much pleased to discover with what confidence the various appliances may be depended upon for securing the exact result aimed at. For example, a very intelligent gentleman, who for a long time suffered from chest disease, had an attack of sickness and diarrhœa. The cold bag was applied in bed to the middle and lower third of the spine. He was able in about a hour to report the removal of sickness and pain; a warm glow over the limbs, and ere long of the entire removal of the diarrhœa. On a subsequent occasion, when the same disorder was brought on by over-fatigue, the same remedy was employed with like success."

I have been informed by Mr. Mardon Wilson, of Philadelphia, that his brother, Dr. Wilson, of that city, has found by experience of many cases that my method of treating *Cholera infantum* is very successful. My own experience in treating the cases of infantile diarrhœa, due to the irritation of teething, has confirmed my most sanguine expectations. In these cases, the particular symptom of spinal irritation, diarrhœa, is not only subdued, but the restlessness, fretfulness, and general irritability of the little sufferer are simultaneously overcome. Soon after the ice is applied along the back, the child usually goes fast asleep with the ice on, and when it awakes is refreshed and calm, the diarrhœa having quite ceased mean-

while. Such being the result in such cases, I feel justified in predicting that if in all instances of preternatural excitability while teething ice were properly and adequately applied along the spine, those disastrous effects of dentition which are now frequent—viz., convulsions, often ending in permanent epilepsy and paralysis, which, though sometimes only temporary, is not seldom persistent, and associated with arrest of growth of the affected limbs—would be wholly averted. I may add that, as in children the vascular system is in its maximum state of activity, ice can be borne by them for a much longer time with impunity than is the case with adults.

[At the beginning of Chapter I., I have referred to “Diarrhœa originated by forms of force hitherto unrecognised as causes of the disease, or the *modus agendi* of which remains unknown.” It seems to me probable, that whatever may be the aspects of such forms of force operating as the proximate causes of diarrhœa, most of them will prove ultimately resolvable into heat. Meanwhile, however, it may be convenient to avail ourselves of the above heading, under which, to range cases of the malady, the causes of which does not at present seem reducible to any one of the four denominations previously mentioned. It has already been ascertained that the condition of the atmosphere with reference to the presence and absence, or excess and deficiency of ozone, sustains a very important relation to the development of epidemic diarrhœa. The relative amount of oxygen, as well as its electric state, also the relative amount of carbonic acid in the air exercise, no doubt, an important influence on the nervous system, and thus on the generation of diarrhœa. Other modifications of the air eluding detection by the most elaborate chemical analysis most probably obtain. What those modifications consist in, what conditions of atmospheric oxygen, what proportion of ozone in the air conduce most effectually to health and disease respectively, are questions still unanswered, and still requiring for their solution a large amount of laborious investigation. It may be, therefore, advantageous and suggestive to conclude this part of my exposition with the heading of a chapter still to be written—a heading which points to the area of the still unknown causes of the malady in question, in which there remains a large sphere for investigation and successful achievement.]



**PART II.:**  
**CHOLERA.**



# CHOLERA.

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## CHAPTER I.

### DEFINITION, HISTORY, AND SYMPTOMS.

*Definition.*—An extremely destructive disease, endemic and epidemic in India, occurring sporadically and as an epidemic in Europe, and most other parts of the world, characterized by vomiting, and purging of a watery fluid, containing an abundance of whitish flocculi; by cramps; by an extremely great and rapid fall of the bodily temperature, with simultaneous and extraordinary diminution of most of the vital functions; by terminating in death, sometimes three or four, and often within twenty-four hours after its first onset, or in reaction, sometimes healthy, sometimes involving dangerous sequelæ, generally of a typhoid nature.

*History.*—“From the contemporaneous writings of Hippocrates, and the Chinese physicians, as well as from those of the later Hindu and Roman medical authors, it is evident,” says Sir Ranald Martin, “that epidemic cholera was well known, and accurately described in their respective times.” Hippocrates has, however, given no detailed account of it. The most distinctly recognisable description of the disease by the ancients, is to be gathered from the writings of Celsus, C. Aurelianus, and Aretæus, and is thus quoted by Dr. Macpherson—“Passing upwards and downwards of bile, variously coloured; at first like water, then like washings of raw flesh; sometimes white, sometimes black, or of various colours. Should the disease increase, the patient grows faint, the muscles of the arms and legs are cramped, the nails grow blue, the extremities cold, the body is covered with sweat, the bladder is spasmodically affected, and the urine suppressed; the voice is lost, and the pulse becomes exceedingly feeble. There is great feeling of heat, and intolerable thirst; hurried breathing; the face is collapsed, and the eyes red towards the end.”\*

It is described in “Charaka,” the most ancient of the Medical

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\* “Cholera in its Home: with a Sketch of the Pathology and Treatment of the Disease.” By John Macpherson, M.D., p. 50.

Sanscrit writings, as follows: "*Cholera Bisuchika*.—The patient first feels pain in the abdomen, as from indigestion, followed by frequent stools and vomiting, great thirst, increased pain in the abdomen, with fainting, giddiness, yawning, and cramps in the legs. The colour of the body is altered, accompanied with shivering pain in the chest and head. The unfavourable symptoms are, great restlessness, no secretion of urine; the lips, gums, and nails, become blackish, with frequent vomitings and faintings. The eyes become sunken, voice feeble, the joints loose, with great debility and insensibility. Such a person will not recover."\*

About 1560, cholera was met with in India, by García del Huerto, a physician at Goa. He described it as appearing chiefly in June and July, and as "killing people in twenty-four hours, sometimes in ten, and at longest in four days."† Nearly seventy years afterwards, or in 1629, Bontius, physician to the Dutch East India Company, described the disease, as he observed it in Batavia. "The cholera morbus," he says, "is extremely frequent: in the cholera, hot bilious matter, irritating the stomach and intestines, is incessantly and copiously discharged by the mouth and anus. It is a disorder of the most acute kind, and therefore, requires immediate application." In its severer form, "the animal spirits" are described as speedily "exhausted, and the heart, the fountain of heat and life, is overwhelmed with putrid effluvia; those who are seized with this disorder generally die; and that so quickly, as in the space of four-and-twenty hours at most. This disease is attended with a weak pulse, difficult respiration, and coldness of the extreme parts: to which are joined great internal heat, insatiable thirst, perpetual watching, and restless and incessant tossing of the body. If, together with these symptoms, a cold and fetid sweat should break forth, it is certain that death is at hand." It is an interesting fact, that Bontius treats of the cholera morbus and spasm, as separate diseases; although he mentions the occurrence of spasm, in the case of Cornelius Van Royen, who "was suddenly seized with the cholera, about six in the evening, and expired, in terrible agony and convulsions, before twelve o'clock at night."‡ It is not improbable that, the disease which he describes as spasm, may be what is now called cholera-sicca.

The next authentic account of cholera I am acquainted with, is that of Sydenham, written therefore, about 200 years ago. It is a fairly accurate representation of the disease, as it occurs almost every summer in England, and shows that, in his time, the majority of its victims were children, a characteristic of the disease still abundantly attested by the returns of the Registrar-General. Sydenham's description is as follows:—"Cholera morbus is limited to the month of August, or to the first week or two of September. Another

\* *Ibid.* pp. 49—50, cited from Wise's Hindoo Medicine, p. 330.

† *Ibid.* pp. 1, 2.

‡ Quoted by Sir Ranald Martin.

similar, but different disease, appears at any time of the year, originating in gluttony or overdrinking, and is cured in the same way.

"Violent vomiting, accompanied by the dejection of depraved humours, difficulty in passing them; vehement pain, inflation and distension of the bowels; heartburn; thirst; quick, frequent, small and unequal pulse; heat and anxiety, nausea, sweat, cramps of the legs and arms, faintings, and coldness of the extremities, constitutes the true cholera, and it kills within twenty-four hours. \* \* \* There is a sort of cholera-morbus, exceedingly fatal to infants. It arises either from overfeeding, or from teething."\*

In the Reports of the Medical Boards of Bengal, Madras, and Bombay, may be found convincing evidence that cholera had prevailed in India as an epidemic long before Europeans became acquainted with its appearance there. In the Bombay Report there is a reference to a Sanscrit work "which leaves very little doubt, that it has not only been long known to the natives, but proves its identity." Sonnerat, mentioned in the Madras Report, and whose observations embrace the period from 1774 to 1781, speaks of "an epidemic disorder which reigns" on the Coromandel coast, and states that "above 60,000 people from Cherigan to Pondicherry perished." In 1781, the Bengal force of 5000 men marching through the Northern Circars, under Colonel Pearse, was attacked, and 700 men died within the first few days. "Men in perfect health dropt down by dozens, and those even less severely affected were generally dead, or past recovery, within less than an hour. Spasms of the extremities and trunk were dreadful, and distressing vomiting and purging were present in all." A disease in all respects similar is reported by Dr. Burke, who was in the Mauritius in 1819, to have prevailed in that island in 1775. From 1769 to 1817 cholera was observed almost every year in different parts of India by English physicians. But in that year it broke out with such extensive and alarming destructiveness, and was so fatal in the Indian army, that the general attention of the civilized world was arrested by it. It prevailed early in the year at Mymensing, Patna, Kishnaghur, Chittagong, and broke out in August amongst the peasants and labourers of the rice swamps and palm groves of the province of Jessore, no great distance from Calcutta. On the 6th November following, the army of Lord Hastings, at that time halting on the banks of the Sind, in the upper provinces of Bengal, was attacked, and in five days 5000 of the soldiers were destroyed; but before its ravages had ceased, 9000 men had been carried off. Of the native population of Jessore, many thousands were destroyed in a few weeks.

Since the date of this terrible devastation, an authentic record of

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\* The Works of Thomas Sydenham, M.D., translated by R. G. Latham, M.D., vol. ii. pp. 266, 7.



the numerous cholera epidemics which have prevailed in various parts of the world, has been kept; and though there can be little doubt that the disease has existed in India from remote times as an epidemic, and has prevailed with more or less severity in other places, England included, during the summer months, and especially in seasons of unusual heat, it seems not less indubitable that the extent of its range, and the frequency of its occurrence, have increased to a terrible and wonderful degree during the last fifty years. It prevailed successively from 1818 to 1827, in Burmah, Aracan, Malacca, Penang, Sumatra, Siam, Ceylon, the Mauritius, and in Chinese Tartary; and during the greater part of the same period it was present in China. Advancing westward, we find it at Muscat and the Persian Gulf in 1821, and in 1822, in Persia, where it reappeared in 1823-29-30. It broke out at Astrachan in 1823, and again in 1830. It appeared in Orenburg in 1829; in 1831 it prevailed in Turkey, Poland, Russia, Germany, Austria (including Hungary), and in October of the same year appeared in England at Sunderland. During the following fourteen months it became epidemic in different parts of the United Kingdom, and destroyed altogether about 20,000 persons. In Paris alone, during 1832, the number of victims of this awful pestilence was 18,000. During the same year it ravaged Montreal and Quebec, and many cities of the United States. Since the great development of the disease throughout Europe and America in 1832, when it also appeared in Mexico and Cuba, cholera has recurred at several periods on both sides of the Atlantic; it now (September 1866) prevails in the United States and many parts of Europe, scarcely any capital city being free from it; and while Paris seems destined to suffer from it this year, at least, quite as terribly as she did last year, London has already lost several thousands through its fatal influence.

*Symptoms.*—In typical cases of cholera, the symptoms present, during the progress of the disease, certain phases which, though merging insensibly into each other, are sufficiently distinct to be recognised as separate stages. These stages denote respectively, the premonitory symptoms, the manifestation of the symptoms indubitably characteristic of the disease, the period of so-called collapse, and that of reaction, or consecutive fever. These several stages are conveniently distinguished as Invasion, Development, Collapse, and Reaction. The testimony of different observers differs considerably respecting the character of the symptoms which first present themselves; this is due partly to the fact that there is great difference of opinion respecting the essential relation of the diarrhœa generally preceding the full development of cholera, to cholera itself, so that the symptoms observable at the onset of the diarrhœa, while being regarded by some, are not regarded by others as constituting the signs of choleraic invasion;

partly also to the fact that a very large proportion of patients suffering from the premonitory diarrhœa are not seen by medical men until the diarrhœa has existed some time. Moreover, the experiences of patients at the very beginning of the malady are vague and indefinite, differing greatly in different cases, and being as they are expressions of disorder of the nervous system, they are, like the symptoms of numerous diseases of that system, exceedingly various, indefinite, and difficult to describe. As I regard the diarrhœa premonitory of cholera, and the epidemic diarrhœa occurring at the same time and place with cholera as identical in nature, and as the first stage of cholera itself, I consider that a true account of the symptoms of cholera can only be given when it comprehends all the phenomena usually accompanying the onset of the diarrhœa in question.

First Stage: *invasion*. Sometimes the first fact observable is slight looseness of the bowels; but this is often preceded and very often accompanied by a sense of general lassitude and malaise: without being able to assign any reason for it, the patient feels the continuation of his work or usual occupation irksome, and evinces more or less indifference to events and circumstances which previously would have excited his interest. Sometimes even before diarrhœa occurs there is slight headache, slight deafness or ringing in the ears, dizziness, slight faintness or syncope, and, generally, muscular energy is impaired. If at this early stage patients are carefully examined, other symptoms of a more definite kind may often be observed before diarrhœa has set in, viz., a small quick pulse; slight acceleration of breathing; a sense of tightness across the chest, particularly across the lower part; a sense of weight at the precordia; wandering pains in the limbs; tremors; slight muscular twitchings, and occasional cramps; unusual paleness of the skin; pinched countenance—the lines of the facial muscles being more observable than in health; unnatural coolness of the tongue and surface of the body; impairment of appetite; nausea, and gripings in the abdomen. At this stage the urine is often notably pale and copious. Dr. Parkes mentions several cases in which a large quantity of pale urine had been passed shortly before decisive choleraic symptoms were manifested.\* Of course all the above symptoms are not observable during the early stage in each case, but some, and the more characteristic, are sure to be presented; while before, simultaneously, or soon afterwards, diarrhœa occurs. This is most frequently painless; in many cases, however, it is accompanied with gripings or abdominal spasms. The stools consist, in the first instance, of natural fæces, rendered more or less fluid; these present the aspect common in bilious diarrhœa, gradually become more watery, and finally, before the advent of any

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\* "Researches into the Pathology and Treatment of the Asiatic or Algid Cholera," p. 72.

other decisive choleraic symptoms, they generally merge into the well-known and characteristic "rice-water" discharges. While indifference and apathy of various degrees characterize cholera from the advent of premonitory symptoms to the period of reaction, the peculiarly insidious nature of the diarrhoea during the first stage lulls the patient into a false security, which is but strengthened by his general lassitude and indifference. Referring to European soldiers under his care in India, Dr. Parkes says:—"The non-commissioned officers of companies had orders to inspect their men continually, and immediately to send to hospital any man who appeared to have a tendency to diarrhoea. The result was, that, except in a few instances, when the attack commenced with appalling rapidity, and with marked or painful symptoms, as cramps or syncope, nearly seven hours elapsed, on an average, from the first stool before assistance was sought for. And this was not owing to any ignorance or inattention on the part of the men or of the non-commissioned officers: men constantly reported themselves sick on account of the most trivial complaints, under the apprehension of cholera, but those really affected seemed to be the last to comprehend the terrible disease which was impending over them; they could not believe that a little purging, unattended by pain, and apparently similar to attacks they might have suffered from before, could be followed by consequences so grave and results so fatal.\* Purging, as stated by Dr. Goodeve and many other observers, commences "very often early in the morning." I have noted this fact in several cases, and also that relapses occur most frequently between two and four A.M.

*Second Stage: development.* The transition from the first to the second stage, is, with respect to most of the symptoms, very gradual and indefinable; there are, however, two facts which, in my opinion, may be held to constitute a decisive line of demarcation; these are, the arrest of the hepatic and of the renal secretion: the alvine discharges contain scarcely any or no bile, and no urine is voided. Vomiting, which generally does not occur until diarrhoea has already become considerable, is now frequent and copious. Cramps, which, as stated above, are sometimes experienced even before diarrhoea occurs, most usually set in after the discharges from the stomach and bowels have assumed the rice-water character: the algide group of symptoms grow simultaneously more and more severe, but in respect to their severity they sustain no relation to the frequency and amount of those discharges. During this stage "the alvine evacuations are copious and fluid. \* \* \* The patient often describes them as rushing from him in a full stream. Often so great is the purging, that he sits sometimes on the close stool or privy until several pints, or even quarts, of fluid



have passed from him. Generally the evacuations are repeated and frequent; they are soon attended with a feeling of exhaustion, so that the patient is glad to get to his bed again. This excessive watery purging characterizes the onset of cholera. It is frequently painless, but not always so; therefore we should not suppose that a patient has not cholera because he has griping and pain. So excessive are these evacuations, that in two or three hours, or less, an ordinary-sized stool-pan will be nearly filled."\* Vomiting is generally a less constant and a less frequently recurring symptom than purging; it is often, however, very persistent. In some cases, the patient only seems to vomit when the stomach is loaded with fluid needing to be discharged; in others, the retching is almost incessant, and terribly distressing. Medicines, and fluids to allay thirst, almost invariably excite the stomach to a fresh effort to eject its contents; hence, while the patient is constantly craving for water, every gratification of his desire is almost immediately followed by vomiting, the distress of which is greatly heightened by his extreme exhaustion. The fluids are ejected from both the stomach and bowels in a peculiar way: they are expelled with considerable force, but the expulsive power which is exerted seems to be mainly restricted to the viscera themselves; there is but little voluntary effort, and the thoracic and abdominal muscles are called into action only to a minimum degree. The amount of the discharges differs greatly in different cases, and the respective amounts ejected from the stomach and from the bowels relatively to each other differ not less notably. Dr. Parkes has given much attention to the character of the alvine discharges, I therefore quote his description of them:—"Occasionally the stool was almost limpid, with a very few shreds of flocculi floating in it. In this case the quantity was usually large, and the period of occurrence was in the early stage. The rice-water stool, with more numerous flocculi, was seen also in this stage. The liquid occasionally was thicker, white, or curdled; sometimes very much like white of eggs partly beaten up. This kind of stool also possessed the peculiar smell more strongly than others; sometimes the white portion was so well mixed up with the watery part as to give a uniform white or milky look to the stool."† Cramps are generally experienced simultaneously with the appearance of rice-water evacuations; they are most frequent in the extremities, especially in the fingers and toes, and in the calves of the legs; they often invade the thighs and abdomen, and occasionally the shoulders and neck. In many cases they recur at frequent intervals, from the time of their onset until reaction sets in, or, in fatal cases, until within a few hours of death. They are

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\* Article on Epidemic Cholera. By E. Goodeve, M.B., in Reynolds's "System of Medicine," vol. i.

Op. cit. p. 53.

the chief cause of active suffering in cholera, and inflict much agonizing pain, over which medicine has little or no control.

While these three leading symptoms—purging, vomiting, and cramps—are gaining ascendancy, the vitality of the patient is steadily lessening; the force and extent of the circulation are becoming more and more restricted; and, simultaneously, respiration, and consequently aëration of the blood, are correspondingly lessened. The supply of oxygen taken into the body being gradually diminished, chemical change and the evolution of heat are to a proportionate extent arrested; the pulse at the wrist gradually loses its strength; the breathing grows increasingly hurried and difficult, with a sense of tightness and extreme oppression of the chest, and the patient tosses himself about, and throws off the bedclothes in his vain efforts to get relief from the suffocating feelings which, as the disease advances, become increasingly intolerable. The temperature of the body continues to fall, and, simultaneously, the skin, already moist, or covered with sweat, becomes clammy, and shrunken, and of a dusky hue. At an early stage the countenance is often very remarkable: it presents an aspect of fixedness or immobility, with a stony-staring look of profound depression and indifference; the eyes are already sunken, and surrounded by a ring of darker hue than that of the other parts of the face. A physician previously acquainted with the choleraic visage will perceive in this aspect of the patient unmistakable evidence of the existence of the disease even before he has assured himself of the presence of other corroborative symptoms.

Third Stage: *collapse*. The distinction between the second and third stages is chiefly one of degree. When the intensity of the disease is no greater than already described, the vital powers are still such as to warrant a confident hope of recovery; but when collapse has fairly set in, or is what is often called “profound” or “complete,” the probability of a fatal termination has greatly increased: the circulation and respiration are still further diminished, the body is deathly cold, and is usually covered with profuse clammy sweat; the temperature in the mouth is from 79° to 88°, that of the axilla from 90° to 97°, and that within the rectum from 100° to 103°—or even 150° Fahrenheit; the skin, previously dusky, has now assumed a leaden or livid hue; the hands and feet are of a dark purple colour—sometimes so dark as to appear almost as if they had been blacklead—an aspect well characterized by the term cyanotic. This appearance often extends a considerable distance up the arms and legs; the fingers are not only shrunken, but seem soddened and corrugated, like those of a washerwoman, or of a person who has been a long time in a Turkish bath. The eyes are now profoundly sunken, and the dark ring around them has become almost black—their movements and expression seem the only remaining signs of life in the ghastly corpse-like countenance; the forehead is generally very cold, the tongue remarkably so;



and the breath, as observed by Dr. Goodeve, is "a cold air stream." The respiration itself is increasingly short, laboured, and difficult; the voice is reduced to a whisper, or becomes extinct; the pulse is flickering, or completely absent at the wrist, and, in very severe cases, is not perceptible even in the brachial artery; the tightness across the chest is increasingly oppressive, and is often associated with an intolerable pain below the left mamma and in the left epigastric region. Thirst is still incessant, and is the one feeling which will prompt the patient to make the utmost efforts to rise in bed in order to gratify it by gulping down the longed-for fluid. The alvine discharges, which now pass from the patient unheeded, gradually lessen, and, in fatal cases, often cease some hours before death. Vomiting also simultaneously declines, but, according to my observations, is more persistent than the purging. The cramps are generally prolonged through the greater part of the period of collapse, are often extremely violent, and sometimes death is ushered in with convulsions. The state of the mind in this stage of profound and general depression, when the patient has the expression of a corpse, is very remarkable, and has always arrested especial attention: the intellect is clear, though enfeebled; there is no approach, unless in extremely exceptional cases, to delirium; the patient understands all that is said to him, and can, if resolved to make the necessary effort, unless in the most severe cases, converse intelligently, but he appears profoundly indifferent and apathetic, it being with difficulty that he can be roused to evince an interest in any subject. In short, while the mind is being steadily weakened, its functions do not seem to be in the least perverted, and the patient remains sensible to the last. Simultaneously with the steady diminution of the cerebral circulation, deafness comes on and increases, and the vision becomes obscure. The duration of the period of collapse is extremely various: sometimes it is only a few hours, sometimes it will extend over two days, or even more. In fatal cases the temperature of certain parts of the body generally rises two or three hours before death. "There is often some return of heat in the scalp and forehead, over the region of the heart, or whole chest, and it may be also over the abdomen; the extremities are still icy-cold, and the cholera visage is unaltered. This partial return of heat on the head and trunk is an immediate forerunner of death, and, as far as I have seen, is invariably a fatal sign; it is occasionally confined altogether to the cardiac region, and is sometimes astonishingly great."\* If recovery takes place, the patient becomes less thirsty, sleeps during short intervals from time to time, and becomes tranquil; the skin loses its clamminess, and becomes dry; the pulse, at first reappearing as a faint flicker, grows gradually

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\* "Researches into the Pathology and Treatment of the Asiatic or Algid Cholera." By E. A. Parkes, M.D., pp. 101, 102.

more distinct ; respiration and circulation improve ; the countenance evinces more life and a more natural expression ; and the returning warmth throughout the body gives assurance that a complete reaction is at hand.

**Fourth Stage : reaction.** The patient may gradually emerge from collapse and recover his health without suffering from any local congestions or general fever : the liver resumes its function ; urine is secreted ; and in some cases the natural strength is regained in a wonderfully short time. But after the extreme contraction of the vascular system which has existed, there is generally a reaction, which, though not necessarily of corresponding intensity, is usually considerable, often attended with danger, and, indeed, is not infrequently fatal. The brain, lungs, intestinal mucous membrane, and kidneys are most liable to become congested, and, it may be, inflamed. Often when the rest of the body has acquired its normal temperature, the head becomes unduly hot, and the patient lies in a state of mental torpor. In many cases, though the brain is greatly oppressed, the patient will answer questions intelligently ; in others, the cerebral congestion is more severe, the conjunctivæ are injected, the eyelids remain half closed, and the patient sinks into a sort of lethargy, the issue of which is very doubtful. Pulmonary congestion is sometimes the chief lesion, and is of course denoted by the impairment of the respiratory functions usual in such cases. Not infrequently the lungs are still more severely implicated. "It is a very interesting circumstance," observes Dr. Parkes, "that pneumonia, or a state approaching to it, should be so often present in consecutive fever, both in Europe and India." When the alimentary mucous membrane is the chief seat of injury, gastro-enteric inflammation of various grades of severity ensues, and is not infrequently of a prolonged and chronic kind. Congestion of the kidneys, proceeding to severe inflammation, and resulting in uræmia, often occurs ; and permanent lesion of these organs resulting from cholera is probably not uncommon. "I once saw a case of chronic albuminuria," says Dr. Goodeve, "which appeared to have originated in an attack of cholera a year previously."

Various other local affections may occur during the stage of reaction, irrespective of the existence of general consecutive fever ; but they are not peculiar to reaction from cholera, and need not be especially described here. Consecutive fever of all grades of severity is frequent. In many cases it is very mild, and may be best described, perhaps, as a state of feverishness ; in others it is truly formidable, assumes the typhoid character, and "may," as Dr. Goodeve observes, "possess all the usual symptoms of adynamic fevers—low delirium, coma, dry tongue, parched lips, feeble and quick pulse, bed sores, liquefaction of the blood, purpura, &c." Sometimes patients, after making considerable progress in the direction of healthy reaction, fall back into collapse, which then often proves fatal. Partial relapse is, however, more common, and generally, of course, has a more favourable issue.

## CHAPTER II.

### POST-MORTEM PHENOMENA IN CASES OF DEATH DURING CHOLERAIC COLLAPSE.\*

1. *The temperature of the body rises immediately after death, and continues to rise for a considerable time ; or if it does not rise the body retains its heat an unusually long time.* "The bodies continued warm for a considerable time, and the peculiar heat of the trunk which comes on occasionally during the last moments of life seemed often to increase after death." (Parkes.) "In Case 2,† during life the coldness of the surface was very marked, two hours and a half after death the thermometer indicated  $102\cdot12^{\circ}$ . In Case 6, ten minutes before death, the temperature in the axilla was  $103\cdot1^{\circ}$ ; five minutes after death it was  $104^{\circ}$ . In Case 9, twenty-five minutes after death, the temperature of the uncovered abdomen was  $89^{\circ}$ ; forty-five minutes after death it was  $90^{\circ}$ ; and fifty minutes after death it was  $91^{\circ}$ ." (Gull.) If the temperature of the body be measured three or four hours before death, and then a full hour afterwards, the increase of heat will, I believe, be found to be greater even than is implied in the above statements; for, as stated in the preceding chapter, when death approaches the temperature begins to rise.

"When an absolute increase of temperature was not observed, the length of time during which the body retained its warmth was in many cases remarkable. In Case 1, five hours before death, the temperature was  $95^{\circ}$  in the axilla; the patient remained in the same collapsed state, and died half an hour after mid-day; three hours and a half after death the temperature in the axilla was  $95\cdot36^{\circ}$ , and in the groin  $97\cdot16^{\circ}$ , being an increase of nearly half a degree above that noted during life. In Case 2, the temperature was  $101\cdot12^{\circ}$  two hours and a half after death. In Case 3, the temperature of the room being  $75\cdot2^{\circ}$ , the body, two hours and a quarter after death, was  $98\cdot24^{\circ}$  in the axilla, and  $100\cdot4^{\circ}$  in the groin. In Case 9, the surface of the abdomen retained its temperature unaltered for forty-five minutes. In Case 10, the heat observed at the moment of death was not only maintained, but an

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\* Dr. Gull's comprehensive and elaborate "Report on the Morbid Anatomy and Pathology of Cholera," and Professor Parkes's careful record of his own observations of the "Post-mortem Appearances in Cases of Cholera," are the chief authorities on whom I shall rely in respect to the facts described in this chapter.

† The numbers here mentioned refer to the cases given in Dr. Gull's Report.



hour afterwards was nearly 2° higher." (Gull.) In a case which occurred in the practice of Dr. Green, of Bristol, the body was not quite cold until full three days after death had taken place. The interesting particulars of this case were recorded by Dr. Barlow, and are cited by Dr. Gull.

2. *The skin often becomes lighter in colour after death, and the shrivelled appearance of the hands and feet diminishes. (Parkes.) Even when the blue colour has existed in a marked degree during life, it often quickly disappears after death. (Sir Thomas Watson.)*

3. *The voluntary muscles of the extremities, as well as of other parts of the body, often begin to contract shortly after death, and continue their movements during different lengths of time in different cases, varying from a few minutes to two hours.*

"The well-known contractions of the muscles often occur to so great an extent, and last for so long a period, as to excite horror in the ignorant, and add in such minds a further mystery to this disease. The muscles affected were principally those of the extremities; but contractions were likewise occasionally observed in all the other voluntary muscles. These movements varied in extent, from flickering and tremulous undulations in a few fibres, scarcely to be seen or felt, to contractions sufficiently powerful to move the limbs from their position, or even to displace the body itself. By a rare coincidence, in which one or more sets of muscles were simultaneously affected, the results had sometimes a voluntary character: thus both arms were, in one case, observed to be flexed, and the hands approximated together as in the attitude of prayer; in another, the eyes opened and moved slowly downwards; and in a third, the right arm was brought over the chest and the hand lifted to the throat. We refer these results to *coincidence* of contraction in different muscles, and not to any co-ordinating influencing of the nervous centres, not only because such an explanation is more in accordance with the whole phenomena, and is in itself sufficient, but because the manner in which the contractions took place, even when they had such a complex character, was altogether different from the action of voluntary muscles under nervous stimulus, being either slow and creeping, like the movements visible in separate fibres under a microscope, or, when more lively, like the twichings seen in the muscles of a slaughtered animal.

"They began shortly after death, and lasted a variable time, from a few minutes to two hours, as in a case observed by Mr. Kesteven, in which the lower jaw was moved at intervals for that period. They were rarely so enduring as this. In the case of a female reported to us by Dr. Parkes, they continued for an hour in the flexors and fingers and fore-arm, cadaveric rigidity at the same time beginning in both knees, and in the elbow of the opposite side, the temperature of the abdomen continuing as high as 91°. When not occurring spontaneously, they could still be excited by percussion or other mechanical stimulus of the fibres.

"They were more commonly observed in those who died rapidly of the disease, in the middle period of life, when the muscular system was vigorous and well developed, which will, perhaps, account for their greater frequency in males than in females." (Gull.) In the *Cholera Gazette* for 1832, it is mentioned that in India the dead bodies of the soldiers were so violently convulsed, that their comrades, "in order to calm the timid, bound the limbs to the bed-frame."\*

4. *Cadaveric Rigidity ensues very quickly after Death.*—"Cadaveric rigidity often supervened very quickly: in one case it began at the end of an hour after death. Briquet and Mignot observed it after forty minutes, and in most cases before two hours had elapsed. Its occurrence was not retarded by the high temperature which the body retained; in one case being very marked, whilst the temperature in the axilla was as high as 100·58." (Gull.) "In all cases the rigor mortis came on very soon after death." (Parkes.)

5. *Duration of Rigor Mortis.*—The comparative duration of rigor mortis does not seem to have been observed with sufficient accuracy to permit of a decisive statement on the subject. Dr. Gull says, "Rigidity occurred not only at an early period, but lasted from twenty to forty hours." Dr. Parkes's evidence is indecisive: "Whether it lasted longer than usual," he says, "I could not determine, as the bodies were buried in many cases, before it had ceased."

6. *Rapidity of Putrefactive Changes.*—The rapidity with which putrefactive changes supervene has not yet been sufficiently observed to permit of a decisive statement on the subject.

#### CONDITION OF THE SEVERAL PARTS OF THE BODY.

7. *The Brain.*—The substance of the brain is normal. The cerebral blood-vessels exhibit extreme venous congestion, and there is not infrequently, as a consequence, some serous effusion. "The morbid appearances in the brain and its membranes were generally indistinct, and were principally venous hyperæmia and serous effusion, varying in degree and amount according to the duration of the symptoms and the mode of death; the pia mater was often œdematous, and the lateral ventricles, in some instances, contained an excess of fluid. The cerebral substance was either natural, or rather watery, but not altered in consistence." (Gull.) "The most usual appearances in the head, consist in the accumulation of blood in the veins of the dura and pia mater, and in the effusion of serum or of blood consequent upon this. This congestion is sometimes as great in recent cases as in cases of consecutive fever with head symptoms." (Parkes.) "There was well-

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\* "The Practice of Medicine." By T. H. Tanner, M.D. Fifth edition, p. 494.

marked venous hyperæmia, with œdema of the pia mater, and sometimes a slight increase of fluid into the ventricles." (Virchow.)

8. *The Spinal Cord*.—Evidence of the condition of the spinal cord sufficiently abundant and distinct to warrant a positive statement concerning it, I have not yet met with. Virchow, quoted by Gull, observes that "he met with no structural changes in the spinal cord." Dr. Bird, in an able paper, read to the Med. Soc. of Victoria, February 7th, 1866, says, "The spinal cord and cerebellum are almost always noticed as congested, and the spinal arachnoid as distended with serum." Professor Parkes did not examine the spinal cord in any of the cases mentioned in his work.

9. *The Sympathetic Nerve*.—The only record I have met with respecting the post-mortem condition of the sympathetic is in Dr. Bird's paper just quoted, and in a paper, mentioned below, by Dr. Beale. Dr. Bird says, "I can find no notice of the condition of the sympathetic ganglia, which is easily accounted for by the fact, that a careful dissection is required to find them, for which observers have but little time during cholera epidemics." He then adds the following note: "Since this was written, it appears that a German physician, Dr. Mayer, has been insisting on the primary influence of the sympathetic system in the productions of cholera, and declares that he has always found disorganization of the ganglia in examining the bodies of cholera patients." Dr. Beale asserts, on the contrary, that "the nerves and the ganglia, so numerous between the muscular and mucous coats of the small intestine, exhibit a natural appearance, so that I should not be able to distinguish a ganglion taken from a cholera victim, from one taken from a perfectly healthy person of the same age, whose life was destroyed by accident. The nerves and ganglia, and the tissues for a short distance around the smaller vessels, are, in many situations, stained with altered and dissolved blood-colouring matter."\* Perhaps the pre-vertebral ganglia only may have been those referred to by Dr. Mayer.

10. *Larynx, Trachea*.—The aspect of these organs is usually normal. "In the majority of cases the parts were unaltered." (Parkes.)

11. *The Lungs*.—The lungs are remarkably collapsed, flaccid, and bloodless, excepting in the lower and posterior parts, which are usually said to be congested. There is a remarkable diminution in the weight of the lungs, which are from ten to twenty ounces lighter than the healthy lungs of persons dying from other diseases. Sometimes the mucous membrane of the bronchial tubes is congested; sometimes they contain frothy serum, and frequently "a white adhesive substance in the smaller tubes, particularly in those in which the irregular remains of the cartilaginous rings begin to disappear." (Parkes.) The pulmonary artery is

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\* Microscopical Researches on the Cholera. By Lionel S. Beale, F.R.S. *Medical Times and Gazette*, August 4, 1866.



distended, the pulmonary veins are empty. As the lungs are usually extremely contracted, the bronchial tubes and air-cells contain, of course, a very small quantity of air; and therefore there is partial or total absence of crepitation. This deficiency of air is not due to any mechanical hindrance to its entrance, for the collapsed lungs can be artificially inflated, the crepitation being thus partially or completely restored. In some cases, parts of the lungs contain frothy serum. The colour varies according to the quantity of blood in the vessels, being palest where there is least, and darkest where there is most. In all cases "the colour not only became redder on exposure to the air, but on inflation it became vividly red. In every instance in which this process was used, the pale lung became redder, and the dark lung lighter in colour. \* \* \* Many writers have described great congestion of the lungs in cholera: the observation seems to have reference chiefly to the quantity of blood escaping from the roots, as on section of the substance of the lung very little blood escapes as a general rule." (Parkes.)

11. *The Heart*.—The pericardium usually contains a small quantity varying from one drachm to half an ounce or even one ounce of serum; generally of a pale straw or citron colour, but sometimes more or less bloody. The coronary veins are generally congested, and when the veins are full the cardiac substance is dark with blood. (Parkes.) The muscular substance of the heart is generally healthy. The right auricle and ventricle, as well as the pulmonary artery, are usually distended with blood; while the left auricle and ventricle, as well as the aorta, are empty, or nearly so.

12. *The Liver*.—The portal and hepatic veins, especially the former, are usually full of dark viscid blood, and the volume of the liver itself is diminished. The organ is paler than natural; it is bloodless, but exhibits no evidence of disease. Its secreting structure on microscopical examination is found to be normal. The gall bladder is usually full of bile, and its ducts often contain a thin colourless mucous fluid, like to that found in the intestinal canal.

13. *The Pancreas*.—No changes, so far as I am aware, are recorded to have been observed in this organ.

14. *The Spleen*.—The evidence concerning the post-mortem state of the spleen is conflicting. Some pathologists describe it as small, contracted, and containing little blood; others say they have found it enlarged and congested; but almost every observer has found great differences in different cases in the size and condition of this organ. "The spleen presented no constant change in respect to volume; \* \* \* in the majority of cases it was normal in size. There were cases in which it was very notably increased. \* \* \* In one instance, where the spleen was of very large size, the white corpuscles were equal to hemp-seeds, of a yellowish white colour, and of such consistence as to admit of isolation from the surrounding splenic tissue. \* \* \* In those who died in the cold stage, it was not rare to find spots of hyperæmia and solid exudation, of a reddish-brown or bluish-red colour, from extravasated blood.

These masses (hæmorrhagic infarctions) were of different sizes, from a hazel-nut upwards. Under the microscope they presented amorphous fibrin and blood corpuscles." (Reinhardt and Leubuscher, quoted by Gull.) Seven times out of twenty-five observations, Briquet and Mignot noted "a slight ramollissement, and six times it seemed voluminous, and perhaps hypertrophied. \* \* \* The consistence of the splenic tissue was increased; it had a tint of lighter red disposed in patches of irregular size upon a violet ground. In one subject there were spots of apoplexy." Leudet says—"Sometimes the corpuscles of Malpighi were apparent. In only one case was there interstitial apoplexy." Dr. Gull says—"In a report of seventy cases examined by Virchow he states that he arrived at no definite results as to the morbid changes in the spleen. He found acute enlargement of this organ in so many instances, and under such variable circumstances, that he could not refer it to mere accident."

15. *The Kidneys*.—These organs are of the natural size and, excepting venous congestion, are free from disease. "The structure was apparently unaltered in every case among the Europeans. \* \* \* In some cases there was a little congestion of the cortical substance; there was often a minute speckled appearance on a pale ground. In some cases there was a white milky fluid in the pelvis of the kidney." (Parkes.) "The kidneys were of the natural size; their surface was mottled by arborescent venous injection; and on section the same venous hyperæmia gave a dark colour to the cones. The secreting structure rarely presented any obvious morbid change. \* \* \* The contents of the pelvis were mostly turbid with exfoliated epithelium and free nuclei. The mucous membrane was generally pale, but in some instances it presented patches of venous injection." (Gull.)

16. *The Urinary Bladder*.—This viscus is always contracted, and often to an extreme degree: it "was always found empty, and contracted into the size of a walnut." (Sir Thomas Watson.) "Contracted in every case, sometimes to an excessive degree." (Parkes.)

17. *The Reproductive Organs*.—In those of the male no change has been observed. Those of the female do not seem to have been observed with sufficient care and frequency to warrant a general and positive statement respecting their condition. Reinhardt and Leubuscher, cited by Gull, state that the whole uterus was sometimes hyperæmic, that its mucous membrane was generally so, and that "its contents were mostly sanguineous, sometimes mucus tinged with blood, and sometimes blood only, either fluid or coagulated." These authors also report the same hyperæmia to have been observable in the ovaries and in the vagina; but whether the hyperæmia in question was venous or arterial is not mentioned. Virchow and M. Leudet (also cited by Gull) found no characteristic morbid condition of the genital organs. The former, however, reports that he found "in the uterus great swelling and hyperæmia of the mucous membrane with enlargement of the uterine glands."

18. *Pharynx and Œsophagus*.—The recorded observations of these



organs are discordant; but, according to Leudet, they "were constantly covered with a soft, pulpy detritus (exfoliated epithelium?)."

19. *The Stomach*.—The stomach is generally pale, and is often distended more or less with turbid mucoid fluid. Its mucous membrane is thickened and opaque, its texture is firm, and the surface, which is mammillated, is covered with tenacious mucus, having in some cases a puriform character, from the large admixture of exfoliated epithelium. Its solitary glands are enlarged. When the surface of the membrane is not pale, its colour is due to hyperæmia, consisting of general arborescent venous congestion.

20. *The Peritoneum*.—The small intestines have generally externally a pink, or rose tint, from hyperæmia of the portal venous system. In some instances they have a remarkably dark colour, the venous trunks being large and full of pitchy blood. The different tints soon change on exposure to the air. The surface of the membrane is covered with a thin layer of a glutinous fluid of a yellow colour, having a pungent, sour smell, and commonly an acid reaction. Sometimes a few drachms of yellow serum are found in the peritoneal cavity; but in no instance is fibrinous exudation, or a vestige of inflammatory action observable.

21. *The Small Intestines*.—The coats of the small intestines are thickened and pulpy. This condition is more especially observable in the duodenum and ileum, and to a greater extent than in the jejunum. The mucous membrane is pale throughout, but generally exhibits venous hyperæmia; occasionally the "arborescent venous injection" extends over large tracts, at other times is observable as patches of variable extent. All pathologists who have made post-mortem examinations of patients who have died in collapse, concur in testifying that the hyperæmia observable in the intestinal mucous membrane is generally venous, and that though there is also sometimes capillary congestion with ecchymoses, there is no vestige of arterial hyperæmia, all the small arteries, including their minutest ramifications, being empty.

22. *The Intestinal Villi*.—The villi are swollen and prominent from œdema, especially throughout the jejunum. Their appearance, and the condition of the epithelium covering them, was minutely described by Böhm during the epidemic in 1832-33. According to his observations the villi had sometimes a swollen and rounded appearance from the presence of fluid enclosed beneath the loosened yet partly adherent epithelium; but more frequently they were quite denuded, or with only here and there a solitary cell upon their surface, indicating the previous disposition of the epithelium, and rendering more obvious the change that had taken place. (Gull.) The accuracy of Dr. Böhm's statements is certified by Dr. Beale's observations. He asserts (by implication rather than directly, however)\* that the villi are denuded of epithelium, and

\* It is a curious fact that, in his papers published August 4th and 18th, respectively, he nowhere states that he has actually observed the villi themselves

gives drawings of "epithelial sheaths" from the villi of the jejunum. He says: "In almost all the cases of cholera I have yet examined there is evidence of chronic structural change in the *tissues* of the intestines, and I think we shall be led to conclude, that in most of the cholera victims important morbid alterations have been going on for months, and in some instances for years, before death. In some cases it is probable, that had the individual escaped cholera, he must have succumbed to some other malady within a short period of time. The columnar epithelial cells often exhibit evidence of chronic change; they seem to be stunted, and in many instances the nuclei are much smaller than in health. In the intervals between their attached extremities one fails to find those smaller and younger cells which in the healthy state gradually grow up to take the place of those cells which are removed and give origin to new cells, which in their turn become developed. So also it is to be observed that the masses of germinal matter so numerous near the surface of the healthy villus are almost absent in many of these cases of cholera. \* \* \* In most cases the fresh cells (constituting the epithelial sheaths) seem to be almost destitute of oil-globules, and many present a shrivelled appearance, as if they had not been very active for a long time before death. There seems, indeed, to be the same sort of difference between some of these cells in cholera and healthy columnar epithelial cells, that is observed between the epithelium of a cirrhotic and that of a healthy liver or kidney."\*

23. *The Glands of the Small Intestines.*—The glands, like the substance of the mucous membrane itself, are swollen and enlarged, and are therefore much more prominent and distinct than normal. "The most frequent of all abnormal conditions of the mucous membranes," says Dr. Gairdner, quoted by Dr. Gull, "is the prominence of the intestinal glands, both of the aggregated and solitary, but especially of the latter." When the glands have acquired a considerable size, they appear as compact, firm grains. The fluid evacuated from them contains merely the normal glandular elements—viz., cells, nuclei, and granules. "Lieberkühn's follicles, which open in the intervals between the bases of the villi, are secreting organs. Now, these follicles, so far from being denuded, are choked with epithelium."†

24. *The Patches of Peyer.*—The changes in these patches are similar to those occurring in the villous surface of the mucous membrane generally, and in the solitary glands. There is no

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denuded of epithelium, he only reports finding the epithelial sheaths in the intestines, and describes the condition of the columnar epithelial cells which he saw. He promises, however, to describe "other and very striking changes in the structure of the affected villi in other communications."

\* Microscopical Researches on the Cholera. By Lionel S. Beale, F.R.S. *Medical Times and Gazette*, August 18, 1866.

† Ibid., August 4, 1866.



evidence that they are the seat of morbid changes peculiar to them in kind or degree. A more or less prominent swelling of the separate follicles, without thickening of the intervening mucous membrane, is generally observable, which, giving to the patches a granular appearance, causes the separate follicles to be readily distinguishable.

25. *The Mesenteric Glands.*—The recorded observations concerning the state of these glands are discordant; but evidence strongly preponderates that no morbid change occurs in them. Briquet and Mignot, cited by Gull, say—"The mesenteric glands, so far as we observed, always appeared white, without increase of size."

26. *The Large Intestine.*—The changes observable in the large intestine are identical in character with those in the small intestines, but less in degree; the mucous membrane is generally thickened, and more or less hyperæmic; whenever it is not pale, the hyperæmia consists of arborescent venous congestion.

27. *The Solitary Glands of the Large Intestine.*—In many instances the large intestine presents nothing abnormal, beyond a greater distinctness of the solitary glands. They are round, more or less strongly prominent elevations, sometimes surrounded by a circle of injected vessels, and often translucent from distension of their cavities. In certain cases, the superficial layer of mucous membrane on the prominent parts of the glands has exfoliated, producing a cup-shaped, or lenticular depression, but entire destruction of them in this stage is never observed.

28. *Contents of the Alimentary Canal, and the Evacuations.*—The colour of the fluid found in the small intestines, "was," says Dr. Parkes, "white subalbid, or of a light-grey yellow, or chocolate tinge. The darker tints were, however, only varieties present in prolonged cases, and probably dependent on a particular condition—viz., the passage, in greater or less quantity, of blood-corpuscles. The fluid consisted of a thinner and a thicker portion, and its consistence varied according to the varying quantity of these component parts. The thicker portion was flaky, stringy, curdy, or clotted; it was not spread uniformly over the surface, but lay in masses here and there, as if it had been deposited from an agitated fluid. Some of these masses, or bundles, often adhered with considerable firmness to the mucous membrane, and were detached with difficulty from between the valvulæ conniventes. It had a peculiar albuminous, or caseous smell, more developed by heat.

"The amount of the clotted flocculent substance, and of the thin fluid, varied so much that it would be impossible to state any average quantity. In one case three pints were measured, but there was sometimes much more than this, and sometimes less. Sometimes the intestines were much distended, and the fluid gushed out on puncture in a jet. \* \* \* There can be no doubt that the true choleraic conjee, or rice-water stools, are a portion of this peculiar fluid, the thinnest parts of which seem only to pass off,



while the thicker clotted portion is in a great measure retained. The representative of it in the stool is to be found in the little white shreds, or flocculi, which give the watery dejection its characteristic appearance. The two component parts of the stools—viz., the white part, and the white flocculi, give similar chemical reactions to those of the serous and the clotted portions of the intestinal fluid. The watery portion is occasionally alkaline in reaction, is incoagulable by heat in the majority of cases, though not in all, and precipitates nitrate of silver. The white flocculi are dissolved by alkaline solutions, and are again precipitated by the stronger acids."

"There can be no doubt," Professor Parkes says, "that the fluid in the intestinal canal and the peculiar stools consist of part of the water and salts of the blood mixed with a proteine constituent." In this last statement he implies that these fluids are not produced by an excessive secretion, but are the result of what he terms exhalation. "Nothing," he says, "is more opposed to my whole course of argument than the idea of an increased secretion in any part of the body being an accompanying phenomenon of the algide cholera. \* \* \* It is obvious that in the slightest cases of even the algide cholera, the changes in the blood are not so great, and the powers of life are not so depressed, as to arrest all the secreting power of the intestinal mucous membrane. In these cases it is very possible that secretion and exhalation may proceed together until the severity of the disease arrests first the one and then the other."

This accurate observer, and justly-respected authority concerning the pathology of cholera, states that he at one time held the opinion that the clotted substance in the intestinal canal is a secretion, but subsequently abandoned it when he became convinced that it was irreconcilable with what was known of the chemical properties of the substance in question, and "with the whole course and progress of cholera." A careful examination of the results of the various researches respecting the substance in question convinces me that there is no adequate reason for discarding the opinion that it is mucus. Moreover, this opinion, as Professor Parkes states, has been expressed by Andral in a note read before the French Academy: he says "that the effused white substance is not fibrine, or any other constituent of the blood, but merely modified mucus." Dr. Goodeve says, "It is possibly some modification of mucus," and that "Gairdner thinks that it presents the reactions of mucus."\* Professor Parkes would, as he admits, find less difficulty in holding the same opinion, were it not opposed to the whole course of his argument; he recognises, however, as just mentioned, that in slight cases of algide cholera it is very possible secretion may continue, and in the next chapter I shall furnish evidence, which I hope he will deem convincing, that in severe cases it is not arrested.

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\* Article on Epidemic Cholera. By Edward Goodeve, M.B., in Reynolds's "System of Medicine," vol. i. p. 159.

Since writing the preceding paragraph I have been favoured (Sept. 13th, 1866) with the following statement by Dr. Andrew Clark, who has had ample opportunities at the London Hospital of studying the phenomena of cholera during the present epidemic, and who is well known as an experienced and accurate microscopical observer, especially of the pulmonary structures both in health and disease:—

“The rice-water discharges, when allowed to stand, separated into two portions—a whitish flaky sediment and a supernatant fluid. The latter, commonly of a neutral tint, but sometimes of a pink colour from the presence of hæmatin, was in general slightly albuminous, and contained molecular matter, a few blood discs, and cells similar to those imbedded in the flakes about to be described.

“The flakes were simply portions of coagulated mucus which had been secreted in excess: many of the smallest were plainly moulds of the villi, or casts of the tubular follicles. Each flake consisted of a basis of mucin, sometimes slightly fibrillated, and having imbedded in it innumerable granules in active movement; cells resembling the colourless corpuscles of the blood; larger spherical nucleated cells in different stages of development and decay, varying in size from 1-1500th to 1-800th of an inch in diameter; compound granule cells; blood discs; and fragments of vegetable tissues.

“Patches of conoidal epithelium were never observed in the bowel discharge of any choleraic during life. After death such patches were found abundantly in the contents of the bowels.”

29. *The Distribution of the Blood.*—It has been already stated, that, as a general rule, the right side of the heart is distended, and that the left is always either nearly or entirely empty; that the pulmonary artery, including all its ramifications, is also remarkably distended, and that the pulmonary veins are empty. Every part of the body, which has now been passed in review, has been shown to be characterized by more or less hyperæmia, always consisting of fulness of the venous radicles. As observed by Virchow, with respect to the distribution of the blood generally, “it was obviously accumulated in the veins and their ultimate ramifications whilst the arteries and capillaries were empty. \* \* \* In the hyperæmic villi [of the intestines] the injection was shown by a microscope to be always in the veins.” According with this statement is that of Reinhardt and Leubuscher: they say, “The large venous trunks and the veins of individual organs were full of blood, whilst the arteries and capillaries were for the most part empty.”

30. *State of the Blood.*—According to the reports collected by Dr. Gull and those of Professor Parkes, it appears that, in a certain proportion of cases, the blood, when removed from the body, is in-coagulable, and that generally it is more dark and fluid, or less coagulable than in other diseases. Dr. Gairdner, however, in his summary of the morbid appearances, states that “the blood is much less affected in its physical characters than is usually supposed to be the case. Its coagulation within the vessels takes place much as in other diseases. In the majority of instances, firm clots are found

within the heart, more or less completely decolorized, and the serum, or non-coagulated portion, contains the greater part of the blood corpuscles. The colour of the blood presents nothing unusual, the epithets "dark" and "venous" being in no degree more applicable to cholera blood *after death* than to that of every ordinary form of fatal disease." Moreover, "according to the observations and analyses made by Dr. William Robertson, of Edinburgh, the fibrin of the blood is usually in large amount and coagulable with great firmness."\*

The results of the chemical researches into the state of the blood have been concisely summed up by Dr. Goodeve,† as follows:—"Chemistry shows that the blood has undergone considerable changes. The analyses most known are those by Sir W. O'Shaughnessy, Drs. Parkes, Garrod, and Schmidt. They show a diminution of water, and a relatively increased proportion of solids. According to Garrod's, taking the maximum of solids in health for males at 240, and for females at 227 per 1000, he found that the total solids in seven cases were 251, 260, 271, 271, 275, 282, and 284. The blood globules and albumen were increased; the alteration in quantity of the fibrine was doubtful, but it was less coagulable, and it was probably altered in quality. Dr. Garrod's observations were made on blood obtained after death from the cavities of the heart and neighbouring great vessels. Dr. Garrod and Dr. O'Shaughnessy differ as to the proportion of salts. The former thought they bore as high, if not a higher proportion than in health; the latter, that they were diminished. Dr. Schmidt's observations were made on blood obtained by venesection. His results show an increased density of the blood, and of the morphological elements in proportion to the duration of the exudation process from the surface of the intestinal canal; a relative increase of the solids in the blood, so that after thirty-six hours of the exudation process they reach to nearly half more than their normal proportion. Schmidt thought that the inorganic salts were diminished in the later stages of the exudation process, so that the proportion of organic matter in the serum was doubled. The specific gravity of the fluid is higher than natural; 1076 to 1081, instead of 1062 to 1060. The blood is sometimes found acid. Dr. Garrod thinks that this is due to the impeded excretion of organic acids. Urea in small quantities is sometimes found in the collapse, and generally in the reaction stages. In the main, all the analyses support each other, showing in the collapse stage a greater consistency of the blood, higher proportion of organic solids of all kinds, with impaired coagulability of fibrine, higher specific gravity, occasional acidity, occasional presence of urea, and an undecided proportion of inorganic matter,

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\* "The Science and Practice of Medicine." By William Aitken, M.D. Vol. i. p. 635.

† Op. cit. p. 161.

but nevertheless, probably, a diminution in the absolute quantity of saline elements. In the reaction stages they show often a considerable quantity of urea and softness of the coagula."

It has been indubitably established by Professor Parkes, not only that, by inflation of the lungs after death, the dark blood in them becomes vividly red, but that when it is taken out of the body, and exposed to the air in thin layers, it does the same. "It is certainly a very singular thing," he says, "that in cholera the blood should retain its power, out of the body, of acquiring a red colour when exposed to the air in thin layers, and yet in the body, that this change, as well as the changes leading to the production of heat, should be interrupted. This certainly looks like obstruction only; but the whole question is very obscure."

A dispassionate review of all the phenomena recorded in this chapter will, as it appears to me, produce the conviction that, though the morbid processes to which they are immediately due are usually referred to chemical changes wrought in the blood by an organic poison, they may be quite adequately, and far more simply, accounted for by the hypothesis that they are consequences of purely physical or dynamic agencies, the character of which will be explained in the next chapter.



## CHAPTER III.

### PATHOLOGY OF CHOLERA.

"If, in certain diseases, there is perhaps sufficient evidence of an influence exerted by different agents upon the cerebro-spinal or the ganglionic system of nerves, the nature of this action in the case of cholera is, in the highest degree, obscure and incapable of expression; and I say this with the highest respect for those writers who refer the changes in the blood to unrecognised conditions in the nervous system. I do not deny such changes; I only believe that some more definite expression must be attached to them before they can be properly discussed."—*Researches into the Pathology and Treatment of the Asiatic or Algide Cholera.* By E. A. Parkes, M.D. Page 128.

1. OF all the theories which have been from time to time propounded, by way of explanation of the aggregate phenomena constituting cholera, none has been found satisfactory; for, not only has no theory led the way to a method of successful treatment, but none has been found adequate to give any approach to a coherent and consistent explanation of the various symptoms of the disease. To understand a disease thoroughly, implies a knowledge of the structural changes which it effects in the organism, how it effects them, by what immediate agency it does so, and how it is originated. When a disease becomes understood in this manner, it is not likely to remain long in the rank of those maladies over which the physician has no power. It is generally admitted, that unless any hypothesis which may be proposed accounts for all the facts comprised within the problem which it attempts to solve, it is unsatisfactory; and that only in proportion to its adequacy to render the whole of the facts intelligible, does it deserve respect. Many theories concerning the ultimate cause of cholera have been promulgated; those which attempt to explain its proximate cause are less numerous. All of them, I believe, ascribe the phenomena of cholera to the presence in the blood of an organic poison, and most of them believe its operation to be of a zymotic nature.

2. Several writers have expressed the conviction, that the force of the hypothetical poison is especially expended upon the sympathetic nervous system. Dr. G. H. Bell, in his treatise on "Cholera Asphyxia," published in 1831, endeavoured to prove that the disease consists in a derangement, or arrest, of those organic processes which were at that time believed to be under the control of the sympathetic. Several other English physicians who had had experience in India,

who wrote about the same time, and who were especially impressed with the spasmodic phenomena of the disease, which are more notable in India than in Europe, considered it, in great measure at least, a disease of the nervous system. But one of the most remarkable expressions of this view is a small pamphlet, published in 1832 by a French physician, Dr. L. Auzoux, which, at page 12, contains the following summary of the author's doctrine:—"Pour moi, le choléra n'est ni une inflammation du tube intestinal, ni une asphyxie du poumon, ni une maladie cérébro-spinale. Je la regarde comme une maladie analogue à l'épilepsie. Le choléra-morbus est au grand sympathique, ce que l'épilepsie est au cerveau."\* Mr. J. G. French, whose work on "The Nature of Cholera Investigated" was originally published in 1835, propounded the doctrine that the "cholera-poison" exerts a special influence upon the heart, either paralysing it, or greatly diminishing its force, "and that the sudden diminution of the bulk of the blood, by the separation of the serum in the rice-water discharges, is the salutary effort of nature to accommodate the quantity of circulating fluid to the diminished power of the heart."† Dr. Davey, in his work on the sympathetic, claims to have shown that cholera is due to a morbid condition of that nerve.‡ Dr. Gull, in his elaborate report from which I have drawn so copiously, says—"The morbid appearances characteristic of cholera are most marked in the small intestine, duodenum, and stomach, and the general symptoms indicate an early and severe depression of the ganglionic nervous centres of these parts." And again, he says—"The depression of the capillary power, the extreme exhaustion of the great ganglionic nervous centres in the abdomen, the passive character of the lesions of the mucous membrane, its normal action being reversed to a fatal exosmosis, are peculiar to cholera." Dr. Copeland expresses a similar opinion. He says—"That the vital energy of the nerves distributed to the respiratory, the circulatory, and the secreting organs, is either uncommonly depressed, or entirely annihilated, is shown by the nature of the characteristic symptoms constituting the malady." And Dr. Goodeve, in his comprehensive and elaborate article on "Cholera," published this year, after saying, "We have to suppose that a poison has entered the blood in some manner, either by the

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\* "Du Choléra-morbus, son siège, sa nature, et son traitement." Paris, 1832. After the report of the "Lecture on Cholera," which I delivered last year (1865) to the English physicians at Paris, had been published in *La Gazette Médicale*, the editor of that journal directed my attention to this original pamphlet, and kindly presented me with a copy of it.

† I avail myself of the concise statement of Dr. Bird, whose paper on "Cholera" has been already referred to, as I have not yet had the advantage of reading Mr. French's work.

‡ "The Ganglionic Nervous System: its Structure, Functions, and Diseases." 8vo. 1858.



channel of the lungs, or intestinal surface," and that as to its mode of action, "we are very much in the dark," expresses himself as follows:—"In the lungs very little of the blood passes freely through them in the algide stage. In the intestines an enormous quantity of certain of the blood elements pass through the capillary walls in the exudation period. In both of these sets of actions, parts of the nervous system appear to be under a morbid influence. In the lungs the muscular fibres of the small arteries seem thrown into a state of contraction. In the intestines a sort of paralysis of the smaller arteries and capillaries seems to exist, much as occurs in the sections of the sympathetic nerve in the neck in Bernard's experiments. That the morbid action in the lungs is of nervous character seems probable, from the absence after death of any discernible mechanical obstacle to the passage of the blood, from the paroxysmal nature of the dyspnoea at first, and from the ease with which the pulmonary circulation is re-established when recovery begins. The nervous character of the actions in the intestines seems probable, from its analogy with the results of Bernard's experiments on the sympathetic, in some of which a section of that nerve in the neck caused the surface of the skin supplied by the vessels under its influence to become bathed in sweat."

Excepting Dr. Auzoux, distinguished by his doctrine, that "cholera is to the sympathetic what epilepsy is to the brain," there is a general agreement of the writers just mentioned in the opinion that the sympathetic nervous system is profoundly depressed, or exhausted. Dr. Goodeve, however, while of this opinion in respect to the nervous centres presiding over the abdominal viscera, and believing them actually paralysed, as effectually as are the cerebral branches of the sympathetic, when the nerve is divided in the neck, recognises the spasmodic condition of the pulmonary structures. All these authors proceed on the assumption that the glandular organs are under the especial presidency of the sympathetic, an assumption which, as I have shown in the introduction to this work, is erroneous; these organs being no more under the control of that nerve, and being related to it in no other way, than is every other structure of the body. When I have developed my own views of the proximate cause of cholera, the criticism which I might here offer on the doctrines in question, will, by implication, be at once easily and completely intelligible. I therefore restrict myself now to pointing out how numerous and weighty are the authorities in favour of the hypothesis, that, to a large extent at least, cholera is a disease of the nervous system. And though they teach that, in so far as the nervous system is affected, it is so in a manner diametrically opposite to that which I shall prove to obtain, their teaching is none the less interesting as a foreshadowing of the truth, which it is the object of this chapter to expound. I shall, however, offer a short comment here on the doctrine of Mr. French,



that the heart is paralysed, and that comment, written without reference to his theory, shall be in the words of Professor Parkes: "In those cases of coma, in which the respiratory movements seem carried on by reflex action at long intervals, the comparatively speaking unimpaired action of the heart is very striking. If in some cases, however, the heart seems to fail, and is, in some post-mortem examinations, found flaccid in all its cavities, these are only the exceptions; they prove that failure in the heart's action may occur as an epiphœnomenon; but their only occasional occurrence also proves the existence of some other condition which arrests the circulation independent of the heart."

3. Among the large number of writers who maintain that in cholera the nervous system is unimpaired, or only slightly and secondarily affected, Professor Parkes stands foremost and most important. "The nervous system," he says, "is often till death free from derangement; there are no lesions of sensation, and, beyond the cramps, no lesions of motion; there are merely the vertigo, tinnitus aurium, and feelings of anxiety attending the early stages of this disease as of several others, and produced, it may be, by the first passage of the altered blood through the encephalic vessels." His own doctrine is, that the blood is poisoned, and that the influence of the poisoned blood is *mainly* exerted in impeding or arresting the circulation in the pulmonary capillaries. He says, "That there is some impediment, or arrest of the circulation, in the capillary system generally, and in the pulmonary capillaries in particular, appears almost certain, and it is by no means improbable, from the whole bearing of the facts, that this is due to a chemical change in the fibrine, and in its mode of combination, consequent on the direct action of the active cause." Professor Parkes, with his usual caution and endeavour after scientific precision, prefaces the discussion of his hypothesis—the expression of which I have just quoted—with the following judicious remarks:—"In continuing the discussion of this question, it is impossible to avoid speculation and conjecture, while our knowledge is so limited and imperfect. But speculation, when recognised as such, seems to me essential to the progress of the inquiry; we constantly inquire what *may be*, when we are yet unable to satisfy ourselves what *really is*. And all that is necessary is to keep up the broad line of demarcation between the observed and the inferred, and not to allow previous inferences to bias future observations. With this proviso, while I venture on an hypothetical arrangement of the phenomena, and on a speculation as to the first effects of the active cause, I do not think I incur the charge of deviating from the strict path of observation and induction." And indeed the whole chapter from which this passage is cited is written in this spirit. It is a model of impartial investigation, careful conscientious reasoning, and of candid recognition of facts which tell against his theory.

4. This theory, after being considerably modified, has been adopted

by Dr. George Johnson. He has taken from it an important element,\* the presence of which was likely to increase the probability of its truth in the eyes of rigorous scientific thinkers, has added to it a proposition† for which there is no ground in either experience or analogy, and has introduced it to the world supported on assumptions which he proclaims as truths. With a confidence alien to the genuine scientific spirit he insists, in the absence of evidence, on the perfect adequacy of his doctrine to account for every phenomenon of cholera and to dictate the only rational method of treating the disease, in a series of arguments, plausible at first sight, and calculated to mislead all who do not think for themselves, but which, when weighed in the balance of scientific experience and accurate reasoning, are found to be wholly "wanting."

Referring to the symptoms of choleraic collapse, Dr. Johnson asks, "What is the pathological explanation of this remarkable train of symptoms?" and then replies, "The one great central fact is this, that *during the state of collapse, the passage of blood through the lungs, from the right to the left side of the heart, is, in a greater or less degree, impeded.*" He afterwards says, "I believe the true explanation of the arrest of the blood in the lungs to be this:—*The blood contains a poison whose irritant action upon the muscular tissue is shown by the painful cramps which it occasions; the blood thus poisoned excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and in fatal cases, entirely to arrest, the flow of blood through the lungs.*"

Within so short a compass as the two foregoing paragraphs, it would be difficult, in my opinion, to find elsewhere so many and such important assumptions destitute of proof. It would be quite as reasonable to say that the powerfully contracted and empty, or nearly empty, condition of all the systemic arteries, excepting the large branches, or the distended condition of the systemic veins, their radicles and main trunks included, is the one great central fact of collapse. What *is* the central fact?—is precisely the question which has long needed, and is still needing, an answer; and Dr. Johnson's assertion that the impediment, in a greater or less degree, to the passage of blood through the lungs from the right to the left side of the heart is that central fact, is wholly unproven.

Again, he says—"The blood contains a poison." This assertion, though made by most of the authoritative writers on cholera, and repeated by almost every one who helps to swell the literature of the subject, is unsupported by any evidence of a satisfactory character; while, on the other hand, a large array of facts may be

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\* Viz., the doctrine "that there is some impediment, or arrest of the circulation, in the capillary system generally."

† Viz., that the blood, which in cholera patients is alleged to be poisoned, "excites contractions of the muscular walls of the minute pulmonary arteries."

advanced in support of the hypothesis that the blood of cholera patients is poisoned in no respect whatever.

Again, Dr. Johnson says, "the irritant action" of this hypothetical poison "upon the muscular tissue is shown by the painful cramps which it occasions." Can he offer a single proof of the truth of this statement? Until he has done so, I reply confidently in the negative. In those diseases in which there is unequivocal evidence of the presence of a poison in the blood—small pox, scarlet fever, or measles, for example—the irritant action upon the muscular tissue is not shown by the painful cramps which it occasions, for it causes none. If, then, cramps do not occur in these signal instances of the presence of an organic poison, it is simply absurd to allege that the cramps of cholera are caused by a poison, the very existence of which remains to be proved. Moreover, cramps constantly occur in cases where there is the most unequivocal evidence that the blood is not poisoned. It has been indubitably proved that pure dynamic derangements of the blood supply of the nervous system, and especially of the spinal cord, are adequate causes of abnormal muscular contractions.

"The poisoned blood," Dr. Johnson says, "excites contraction of the muscular walls of the minute pulmonary arteries," but he tenders no proof whatsoever of this assertion, nor does he even supply reasons which render it seemingly probable. Having stated that the walls of the pulmonary arteries are muscular, and that under the influence of a stimulus they have the power of contracting upon their contents, he at once jumps to the conclusion that in cases of cholera they are irritated by a poison in the blood, and consequently, by contracting, impede the passage of blood through the lungs. He states that the injection of a concentrated solution of a salt of soda, or a few grains of nitrate of silver, into the jugular vein, or the admission of air through a wounded vein in the neck or axilla, is speedily followed by death; and that beaten bullock's blood, mixed with air, requires nearly twice the pressure to drive it through the pulmonary vessels that suffices to drive unmixed blood through them. Having thus shown "that the movement of blood through the lungs may be quickly arrested by the addition of some foreign ingredient to the blood, and that this arrest is probably due to the power which the arteries possess to contract upon their contents,"\* he implies that in doing so he has adduced a proof that in cases of cholera the contraction of the pulmonary arteries is due to the exciting influence of a poison in the blood. I submit that there is here no connexion between the premises and conclusion; and that, until the missing link is supplied, the very foundation of Dr. Johnson's hypothesis must be held to be a mere assumption. If the hypothetical poison be, as alleged, an efficient cause of the contraction of the pulmonary arteries, and of the cramps

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\* The reason given for "this arrest" is more than questionable.



of the voluntary muscles, how comes it that Dr. Johnson does not teach that the systemic arteries are contracted by the same agency? If the blood be poisoned, surely its poisonous influence must pervade the whole body; and pre-eminently, according to his own statement, the muscular structures, and therefore the muscular coats of the systemic arteries. In restricting the action of the assumed poison as he has done, he has, it seems to me, weakened his own case; for that the systemic arteries are in a state of intense spasmodic contraction, is believed by at least some competent observers. He says himself—"I suppose that no physiologist at the present day would deny that spasm of the arteries is as real a fact as spasm of the muscles." It is, therefore, difficult to understand how, in presence of this fact, of his hypothetical blood poison, and its assumed irritation of muscular fibre, he does not extend the influence of this poison to the systemic arteries.

Having stated his doctrine in the manner now described, Dr. Johnson says—"We can now understand the sudden coming on of collapse, and its sudden passing off. Robust men falling down 'as if they had drunk the concentrated poison of the upas tree,' and recovering again almost as rapidly as 'patients who are resuscitated after suspension of animation from submersion in water.'" But such 'patients' have not absorbed a poison, and it is not difficult, therefore, to understand their rapid resuscitation. If, however, cholera collapse be induced by a blood poison sufficiently virulent to irritate, and thus to produce contraction of the pulmonary arteries in the manner he alleges, it may be easy to understand "the sudden coming on of collapse," but it is almost, if not quite, impossible to understand "its sudden passing off." In those cases of disease in which we know an organic poison is present, its sudden passing off has never been observed; and as such a fact is contrary to all experience, its possibility is discredited.\* Moreover, it is precisely in those cases in which the alleged contraction of the pulmonary arteries is especially intense, or, in other words, in which the spasmodic phenomena are markedly predominant, and in which the eliminative processes, vomiting and purging—regarded by Dr. Johnson as Nature's method of getting rid of the poison—have been least active and continuous, that recoveries, when they do occur, are most rapid.

"The most interesting and conclusive evidence," says Dr. Johnson, "that arrest of blood in the lungs is the true key to the pathology of choleraic collapse, is to be found in the simple yet complete explanation which it affords of all the most striking chemical phenomena of the disease—the imperfect aëration of the blood, the fall of temperature, the dark and thick appearance of the blood, and the suppression of bile and urine." Now this arrest of blood considered as the central fact of collapse, is nothing more nor

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\* It may be alleged that the rapid recoveries from the poisonous effects of ether, chloroform, and hydrocyanic acid countenance Dr. Johnson's statement; but it must be recollected that these are not *organic* poisons.

less than suffocation, or asphyxia; but, in asphyxia, the characteristic phenomena of choleraic collapse are never induced: the features are not shrunk, but tumid; and the body generally, though more or less livid, is in no respect shrunk, as it is in algide cholera. The differences, in fact, between the two states are numerous and essential. In asphyxia, the liver and kidneys are gorged with blood, but in choleraic collapse they are both anæmic. Without comparing the two conditions in detail, I will merely state a few conclusive facts. *First*, whereas in cholera 'a mortal coldness' comes over the body with astonishing rapidity—sometimes in a few minutes, in asphyxia there is no such rapid diminution of heat—the long continuance of the animal temperature, even after the heart has ceased to beat, is a characteristic fact. *Second*, whereas in cholera cadaveric rigidity comes on with extreme rapidity, in asphyxia it continues absent "almost always for a much longer period than from death under other circumstances, and from other proximate causes."\* *Third*, whereas in choleraic collapse there is suppression of bile and urine, in asphyxia there is neither one nor the other. Thus it appears, that of the several phenomena stated by Dr. Johnson, in the passage last quoted, as being explained by his doctrine, only one—viz., the imperfect aëration of the blood—is really accounted for; and when this occurs in cases of pneumonia so severe as to cause the blood in the systemic arteries to become dark, no symptoms like to those characteristic of choleraic collapse are observable.

Dr. Johnson says—"Suppression of bile and urine during the stage of collapse is a necessary consequence of a limited supply of oxygen, which results from obstruction in the lungs." If so, why is there not a like suppression in asphyxia, and severe cases of pneumonia? If the blood in choleraic collapse contains a supply of oxygen so limited as to preclude the possibility of any oxydation whatever, in either the liver or the kidneys, and thus to arrest entirely the functions of those organs, how is it that there is nevertheless sufficient oxygen in the blood to enable the cerebral functions, which involve a considerable amount of oxydation, to continue without interruption? How is it that when collapse is most profound, and when death is approaching, there is still sufficient oxygen in the blood to permit of a recommencement of oxydation, resulting in a notable increase of animal heat, so that the temperature of parts of the body rises before death, and that of the whole body after death, the elevated temperature being prolonged for a considerable time?

Dr. Johnson accounts for the admitted fact, that in some cases of choleraic collapse "*the secretion of milk continues apparently undiminished*," by saying,—“The chief constituents of milk, casein, sugar, oil, and water, may be obtained from the blood, without the addition of oxygen. The secretion of milk, therefore, continues during the stage of collapse.” What I have already said, con-

\* Copland's "Dictionary of Practical Medicine." 1866. ART. Asphyxy.



cerning "the defective supply of oxygen" as the alleged cause of the suppression of bile and urine, renders Dr. Johnson's ingenious explanation of the continued secretion of milk extremely questionable, and will probably be considered as completely confuting it.

While the positive side of Dr. Johnson's theory is so demonstrably erroneous, the negative side is not less remarkable by the number of phenomena characteristic of cholera which it fails to explain. Of the prodromata of cholera—slight headache, slight deafness, singing in the ears, dizziness, faintness or syncope, and the copious discharge of pale urine—he renders no account, and makes not the least attempt to show how they are produced. His great discovery that vomiting and purging are "the salutary efforts of Nature, by which the morbid poison is eliminated," is not sufficient: if his doctrine be true, it should surely explain the *modus operandi* of these important processes. The fact of the contraction of the bladder to the size of a walnut he passes quite unheeded. The seemingly paradoxical fact, that while the systemic arteries are almost wholly emptied of blood during collapse, sweat is freely, and very often profusely, secreted, also passes unnoticed as well as unexplained. The remarkable calmness and clearness of the intellect during collapse—notwithstanding the suffusion of the brain with blood, the supposed poison of which is said to cause the pulmonary arteries to contract so vehemently as to impede or arrest the circulation, and thus to cause death—receive neither comment nor explanation. The rise of the temperature of the body immediately before death, and for some time afterwards, and the astonishing muscular contractions, often involving movements of the limbs, sometimes even of a seemingly co-ordinated character, after death obtain no recognition whatever.

If it be admitted that a poison exists in the blood, and that it effects the contraction of the pulmonary arteries in the manner stated by Dr. Johnson, his hypothesis does not account for the contraction of the bronchial tubes and the consequent absence of air in the lungs, which is denoted by the extreme collapse observable in these organs. So extreme is this collapse, that, as stated by Dr. Parkes, "several observers have opened bodies under water, believing that air must have been generated in the pleural cavities; but this was found not to be the case, and the shrinking of the lungs must consequently depend on some cause existent in itself." This very interesting and remarkable fact, wholly inexplicable, I apprehend, by Dr. Johnson's theory, is passed over by him in silence. Dr. Parkes feels the difficulty and expresses it.

There is yet another difficulty connected with the lungs: some time before death, usually two or three hours, there is often, as already stated, "some return of heat in the scalp and forehead, *over the region of the heart or whole chest*, and it may be also over the abdomen; the extremities are still icy-cold, and the cholera visage is unaltered." Now it is fairly presumable that when the

supposed poison in the blood is on the point of destroying its victim, it must, according to Dr. Johnson's hypothesis, exert its maximum of poisonous energy on the muscular coat of the pulmonary arteries; and yet at this very time it appears that the poisonous blood often courses through the lungs again, and produces, in consequence, a considerable elevation of temperature. Dr. Johnson fails to explain this very curious and interesting fact, seemingly adverse to his doctrine.

The kidneys are in a very pre-eminent degree the organs by which poisonous elements in the blood are separated from it, and thus cast out from the body: if vomiting and purging be "the salutary and curative efforts of Nature" by which the cholera poison is eliminated, the fact that Nature does not avail herself of the chief eliminating organs—the kidneys—in order to effect a more rapid and complete expulsion of the poison from the system, is certainly very astonishing and inexplicable; but Dr. Johnson ignores this fact also, or, at least, makes no attempt to reconcile it with his doctrine of elimination.

Tried by the test of practice, Dr. Johnson's theory is found to be no less unsatisfactory, than it has been proved to be by the evidence given above. He says: "I maintain that this pathology is a compass which will guide the intelligent practitioner over a 'sea of troubles' and perplexities in the treatment of cholera." Guided by this compass, Dr. Johnson has discovered, that "the natural method of cure is eliminative," and that the best eliminative method, consists in the administration of castor oil. In section 13 of the next chapter, will be found a considerable body of evidence, proving that the practice of giving purgatives to cholera patients, is an extremely dangerous one, liable, in fact, to induce collapse and destroy life; and in the same section, the reasons of their dangerousness are explained. The experience and reasons there given are remarkably accordant with the conclusions expressed in the report on the results of different methods of treatment pursued in epidemic cholera, addressed to the President of the General Board of Health, by the Treatment Committee of the Medical Council of the Royal College of Physicians. The results of the several plans were tabulated so as to show the *per centage* of deaths following each plan. That following the use of eliminants, was greatest of all, viz. 71·7; the per centage of deaths following the treatment by castor oil, was even greater than that which followed the use of eliminants in general; *it was 77·6 per cent.* In the report on the treatment of cholera drawn up for the Royal College of Physicians by Dr. Gull, he says,—“The popular theory that the discharges are an effort of Nature to throw off a *materies morbi*, is not only unsupported by any known facts of the disease, but when applied to practice, is found to increase the violence of the symptoms.” “We have only to say,” observes the *Lancet*, of June 2, 1866, “that there is a discrepancy between Dr. Johnson's experience, and that of the profession. His treatment was largely tried in 1854, and generally and strongly disapproved.”



Considering these decisive statements, the reader will scarcely be surprised by the following confession, published in 1866 by Dr. Johnson himself: "I have not the faintest hope or expectation that a specific remedy for such a disease as cholera will ever be discovered."\*

Having thus estimated as completely and as fairly as I can, within the short compass of a few pages, a theory which has attracted general attention, and which has undoubtedly exerted some influence on practice, I have, I believe, shown that there is still ample room, indeed pressing necessity, for a new investigation into the pathology of cholera: I now proceed to state the result of my own inquiries.

5. I must observe *in limine*, what indeed has already been stated by Dr. Bird,† that the pathological doctrine which I am about to explain does not at all preclude the supposition of a specific poison as the cause of cholera, although, if such a poison be operative, it presumes its influence to be mainly concentrated on the nervous system. I do not affirm, neither do I deny, the existence of such a poison; but I maintain that no trustworthy evidence of its presence has yet been tendered by those writers who assert its existence most positively; and I believe that those subtle agents which produce the sudden and wide-spread developments of epidemic cholera operate directly upon the nervous system, and not through the intermediate agency of the blood.

It is commonly observed, that our first impression of persons with whom we become acquainted are the truest; and experience often justifies the observation. When English physicians first became acquainted with cholera in India, where it presents itself in its most formidable aspects, and before they had formulated any theories concerning it, they were most deeply impressed by its spasmodic character, and hence, regarding the nervous system as primarily affected, designated the disease *cholera spasmodica*. I shall endeavour to show within the remaining pages of this chapter, that though since then pathologists have made a wide circuit, searching in every element of the body for the essential nature of the malady, their first impressions were the true ones, and that for typical, algide cholera, the name they gave it is the best.

6. The pathological doctrine, the truth of which I hope to prove to the satisfaction of every competent judge, may be concisely embodied in the four following propositions:

- (I) All the phenomena of cholera are due to simultaneous hyperæmia of the spinal cord and of the sympathetic nervous system.
- (II) All the phenomena of cholera are naturally divisible into two classes, accordingly as they are referrible to the spinal cord, or to the sympathetic ganglia as their cause.

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\* Notes on Cholera, p. 87.

† *Australian Medical Journal*, March, 1866, p. 81.



(iii) All active or positive phenomena are due to hyperæmia of the spinal cord.

(iv) All passive or negative phenomena are due to hyperæmia of the sympathetic ganglia.

7. The symptoms of cholera are ranged in the following lists as positive or negative phenomena according to my conception of their proximate cause :

POSITIVE PHENOMENA.

- a.* Abnormally copious and pale urine.
- b.* Albuminous urine.
- c.* Abundant secretion of bile.
- d.* Abundant secretion of pancreatic juice.
- e.* Borborygmi.
- f.* Excessive activity of the mucous membrane and of all the glands of the alimentary canal.
- g.* Abnormally high temperature within the rectum.
- h.* Excessive activity of the mucous membrane of the gall ducts and gall bladder, of the pelves of the kidneys, and of the female genital organs.
- i.* Abdominal gripings.
- j.* Excessive expulsive activity of the stomach and bowels.
- k.* Simultaneous excessive activity of thoracic and abdominal muscles.
- l.* Sweat in all its grades of copiousness, and fluidity.
- m.* Derangements of sensibility.
- n.* Tremors.
- o.* Muscular twitchings.
- p.* Fixed, stony expression of the face.
- q.* Tonic hardness of muscles.
- r.* Tightness across the lower part of the chest.
- s.* Cramps and convulsions.
- t.* Extreme contraction of the urinary bladder.
- u.* Restlessness and tossing of the limbs to and fro.

NEGATIVE PHENOMENA.

- a.* Slight headache.
- b.* Deafness of various grades.
- c.* Tinnitus aurium.
- d.* Dizziness, slight faintness, syncope.
- e.* Drowsiness, sleeplessness.
- f.* Mental state.
- g.* Absence of tears, saliva, bile, and urine.
- h.* Short, struggling, and rapid respiration.
- i.* Cold breath.
- j.* Enfeeblement of the voice, aphonia.
- k.* Oppression and burning pain at precordia and left epigastric region.
- l.* Algide symptoms : Progressive changes in the visage, and in the colour, temperature, and general aspect of the skin.
- m.* Loss of cutaneous sensibility.
- n.* Serous exudation into the intestines.
- o.* Epithelial exfoliation of the intestinal villi.
- p.* Enfeeblement and death of the voluntary muscles.
- q.* Enfeeblement and death of the involuntary muscles, cessation of discharges, secretion still continuing.

I shall now advert successively to each of the phenomena above enumerated, in order to explain the *modus operandi* of their production, and will discuss the *positive phenomena* first.

#### POSITIVE PHENOMENA.

8. (a) *Abnormally Copious and Pale Urine*.—I have noticed this symptom in one case, but so far as I am aware, Dr. Parkes is the only author who has recorded it as occurring during the period of *invasion*. Assuming the doctrine of glandular innervation, explained in the introduction, it follows that hyperæmia of the spinal cord will produce excessive secretion of urine so long as the renal arteries remain sufficiently dilated to permit of an adequate supply of blood to pass through them. A moderate excess of nervous, as of saline, stimulation of the kidneys, if they are in a normal state when the excessive stimulation begins, will only increase their functional activity, and will not produce either congestion or inflammation. While they are thus rendered abnormally active, the processes of textural change in the body generally during the onset of cholera are already lessened; the consequence is, that the amount of the elements resulting from the destructive metamorphosis of the organic tissues and appearing in the urine is less than before. Thus it is that the products of the increased activity of the kidneys caused by the more intense stimulus of the positive motor nerves is manifested by an abnormally large flow of urine containing an abnormally small proportion of solid constituents. I have satisfied myself, by a large number of observations, that an excessive secretion of pale urine denotes hyperæmia of the lower segments of the spinal cord.

9. (b) *Albuminous Urine*.—This symptom is observable during the period of *invasion* in some cases, as well as during the period of *reaction*. In the former case the activity of the positive motor nerves of the kidneys has become considerably more intense than it was when they only caused the secretion of urine in excessive abundance. Now, however, the hyperæmia of the lower segments of the spinal cord, and consequently the intensity of the nervous influence transmitted from it through the positive motor nerves to the kidneys, are so considerably augmented that their secreting cells draw blood to them in excessive abundance, thus causing renal hyperæmia or congestion, and albuminuria as a consequence. But soon the supply of blood to the glands becomes gradually lessened; the congestion subsides; the secretion of albuminous urine is arrested, and may or may not reappear during the stage of *reaction*, according to the intensity of the febrile condition accompanying that stage.

10. (c) *Superabundant Secretion of Bile*.—During the premonitory diarrhœa of cholera, the alvine flux contains an abundance of bile. In this stage the positive motor nerves, presumed to be branches of the phrenic, distributed to the secreting cells of the liver, are stimu-

lating them unduly. They consequently, so long as they continue to receive an adequate supply of blood, elaborate bile superabundantly.

11. (*d*) *Superabundant Secretion of Pancreatic Juice*.—I am not aware that any positive proof of the excessive activity of the pancreas during choleraic diarrhœa has been obtained. As, however, the amount of bile and of mucus is increased, I infer analogically that the feculent discharges are partly constituted by an excessive flow of the pancreatic juice. If this be the case, the superabundant secretion, like that of the other glands, is due to the excessive stimulation by the positive motor nerves distributed to the pancreas.

12. (*e*) *Borborygmi*.—Rumbling of wind in the bowels is due to two causes. First, that effecting its secretion there; and second, that producing rapid vermicular action of the intestinal tube. I shall advert in this paragraph to the first only of these causes; the second will be afterwards referred to. What is the precise manner in which the intestinal mucous membrane effuses air, and that very often with astonishing suddenness and abundance, I am not able to explain; but this I know, that it is a symptom very frequently associated with hyperæmia of the spinal cord and sympathetic ganglia; that it can be artificially produced by the application of heat along the lower half of the spine, and that by the application of ice along the same region its production can be arrested, and a condition favourable for the absorption of that which has already been produced can be established. During the generation of air in the intestines the temperature of the abdomen is, as a rule, considerably lower than normal; and thus it appears that in respect to the secretion or effusion of intestinal gases, the same apparent paradox presents itself, which, in the introduction, I have shown to obtain with respect to the secretion of sweat and mucus, while the nervous centres, both spinal and ganglionic, are hyperæmic. The foregoing facts constitute strong evidence that the production of air in the intestines, when not arising from chemical changes of the alimentary or fecal contents of the tube, is due to hyperæmia of the spinal cord.

13. (*f*) *Excessive Activity of the Mucous Membranes, and of all the Glands of the Alimentary Canal*.—It is remarkable, as already intimated, that nearly every writer on cholera, when adverting to the state of the nervous centres related to the intestines, speaks of them as being profoundly depressed or exhausted, and considers that the proper functions of secretion are arrested.\* It is probable that the large amount of serous or watery effusion which occurs has concentrated attention upon itself, and, to the eyes of the majority of

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\* "Paralysis of the sympathetic nerve, and want of the performance of the organic functions, with deficient vitality of the mucous membranes, are Dr. Sedgwick's ideas of the pathology" of Cholera, says *The Medical Times and Gazette* (of Sept. 8, 1866) in its notice of the "re-issue" of his work, "On the Nature of Cholera, as a Guide to Treatment." I have glanced through the work, but failed to find any precise statement of the author's theory.

observers, has masked the important fact that the actual secretion of mucus is enormously increased. The large amount of whitish flocculent matter constituting the characteristic element of the so-called rice-water evacuations has, however, been the subject of elaborate and prolonged researches and discussions, and has given rise to a great diversity of opinion as to its essential nature. Professor Parkes, as already stated, considers "that the fluid in the intestinal canal and the peculiar stools consist of part of the water and salts of the blood, mixed with a proteine constituent;" while, as he himself says, Andral asserts "that the effused white substance is not fibrine, or any other constituent of the blood, but merely modified mucus." Andral's opinion derives strong confirmation from the researches of Böhm, who was the first to point out that the insoluble white flocculent matter contained in the alvine discharges of cholera patients, as well as in the fluid found in the intestines after death in choleraic collapse, consists in part of epithelial cells in their various stages of development, separated or adhering together. "Böhm's statements," says Dr. Wood of Philadelphia, "have been confirmed by subsequent microscopical observers."\* Moreover, his assertion, that the whitish layer which is often found on the surface of the alimentary mucous membrane also consists mainly of the disintegrated epithelium of that membrane, "has been confirmed by a Committee of the College of Physicians of Philadelphia, appointed to investigate the condition of the mucous membrane of those dying of cholera."† Whatever may be the value of Dr. George Johnson's theory, already discussed, I have the utmost respect for his facts, and gladly avail myself of them. Of the nature of the choleraic discharges he has been a careful observer, and he says, "The flocculi in the rice-water stools consist almost entirely of perfectly organized epithelial cells, most of them of large size. Of this fact I have satisfied myself by repeated examinations of the discharges from different patients. The peculiar creamy viscid secretion, which sometimes nearly fills the small intestines after death, is almost entirely made up of the same fully formed epithelium. Now, it is obvious that this large amount of epithelium cannot be explained by the peeling away of one or two layers of cells from the surface of the mucous membrane—the result of a local irritation during life, or of maceration by the fluid contents of the bowel after death. This abundant cell-formation can result only from a very active vital effort. And if the object of that cell-growth be not to withdraw from the blood some morbid products—some constituents of the blood or of the tissues which have been damaged by the morbid poison—it is difficult to suggest any explanation of the phenomenon."‡ I entirely concur in Dr. Johnson's opinion, that this large amount of epithe-

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\* Op. cit., vol. i. p. 682.

† Ibid., p. 685.

‡ Notes on Cholera; its Nature and Treatment, pp. 81, 2.

lium is not produced by the mere "peeling away of one or two layers of cells from the surface of the mucous membrane—the result of a local irritation during life, or of maceration by the fluid contents of the bowels after death;" and that "this abundant cell formation can result only from a very active vital effort." I dissent entirely, however, from his final-cause doctrine—that the object of this extraordinarily energetic and profuse cell-growth is "to withdraw from the blood some morbid products; some constituents of the blood or of the tissues which have been damaged by the morbid poison." I regard it as simply a disastrous consequence of the baneful influence which has been exerted on the nervous centres. Their excessive functional activity is felt and expressed differently in the different parts of the organism according to their different natures and functions. It would be just as reasonable to affirm that the intense spasms of the arteries due to the same cause as is the excessive cell formation, and resulting in collapse, is "a salutary effort of nature," as it is to ascribe the excessive cell formation itself to such an effort.

The really important question is, How are the epithelial cells, which are found so abundantly in the evacuations, and in the intestines after death, produced? In the preceding chapter it has been shown that the intestinal villi are found to be denuded of epithelium, or their epithelial coverings are raised up by serous infiltration beneath them, and are thus in the course of exfoliation. But such exfoliation is clearly not adequate to account for the large amount of cell-detritus which is observable. Dr. Beale, however, seems to think it is, and goes so far as to affirm, that "there is no evidence of the increased formation or more rapid removal of the secreting epithelium in the various glandular organs." I submit, however, that this statement is not justified by observation. Indeed, he reports a fact which I have mentioned in the preceding chapter, but which I recall here, completely subversive of that statement. He says, "Lieberkühn's follicles which open in the intervals between the bases of the villi are secreting organs. Now these follicles so far from being denuded are choked with epithelium." Here then is a source of the epithelial cells in question, quite independent of the exfoliation of the intestinal villi; and if these follicles of Lieberkühn secrete so energetically as actually to choke themselves up with their own débris, it is reasonable to suppose that the other glands of the alimentary canal are stimulated to undue activity in the same manner. Indeed Dr. Beale has since said,—"In many cases there is very little or no columnar epithelium from the small intestine to be found in the stools; epithelium from the surface of the large bowel and from its follicles, and mucus from the surface, being alone present."\* Dr. Clark's evidence, already

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\* Microscopical Researches on the Cholera. *Medical Times and Gazette*, September 22, 1866.

given at the end of section 28 of the preceding chapter is to the same effect, for he states that the whitish, flaky sediment of the rice-water discharges simply consists of "portions of coagulated mucus which had been secreted in excess," having imbedded in them innumerable granules, and large, spherical, nucleated cells, in different stages of development and decay. Moreover, he makes this not less emphatic than decisive statement: "Patches of conoidal epithelium [epithelial sheaths of the intestinal villi] were never observed in the bowel discharge of any choleraic during life." Now, in sections 19, 23, 24, and 27 of the preceding chapter, abundant evidence is presented that the whole of the glandular structures of the alimentary canal are remarkably enlarged: I repeat the words of Dr. Gairdner,—"The most frequent of all abnormal conditions of the mucous membranes is the prominence of the intestinal glands, both of the aggregated and solitary, but especially of the latter." The subject in question is in its bearing on the pathology of cholera so supremely important, that I shall be excused for adding to the evidence already given in support of the assertion that the intestinal gland-cells are preternaturally active during the period of collapse. "Enlargement of the intestinal follicles, and elevation of the elliptical patches are," says Dr. Wood,\* "very commonly met with. \* \* \* In many cases an eruption of minute semi-transparent vesicles has been noticed, disappearing upon puncture, very closely arranged, and extending from the duodenum to the ileo-cæcal valve, and even into the colon. These have been considered as morbidly developed follicles. \* \* \* A glairy mucus sometimes exists in the stomach, more or less adherent to the mucous coat." Dr. Macpherson observes: "The mucous membrane usually presents throughout the whole of its extent prominences of small, hard, opaque bodies, which are enlarged Brunner's and solitary glands. This is a very constant appearance in cholera."† Moreover, not only is the inner surface of the stomach found after death to be covered with tenacious mucus, having in some cases a puriform character, from the large admixture of exfoliated epithelium, but the mucous membrane of the pharynx and œsophagus is constantly found covered with a soft pulpy detritus (mucus and epithelium); and the mucous membrane of the whole alimentary tract, but especially of the stomach and small intestines, is thickened and opaque, the texture of the gastric mucous membrane being firm and its surface mammillated. In presence of this large array of facts it is impossible to avoid the conclusion, that the whole glandular system of the alimentary canal is in a state of the most energetic, tumultuous activity; that the development of gland-cells, which are shed in quick succession, is extraordinarily exuberant; and that a very large proportion at least of the cells and cell-detritus, as well as the whole of the superabundant mucus found in the evacuations and the intestines after death, is the product of this

\* Op. cit., p. 684.

† "Cholera in its Home," p. 57.

excessive development from the vast extent of the glands and glandular surface in question.

A recognition that the functions of this large body of secreting cells are thus enormously augmented and intensified, produces and enforces the conviction that the nervous influence distributed to the entire glandular surface must have been overwhelmingly energetic and intense. My discovery that the mucous membranes generally may have their functional activity increased by the application of heat along the spine, and lessened by cold along the same region, and the evidence which I have given briefly in the introduction that all glands are supplied with positive motor nerves from the cerebro-spinal system, are in perfect accordance with the above facts; and while revealing how it is that, simultaneously with general spasmodic contraction of the arterial system, such immense outpourings from the mucous glands of the alimentary canal are possible and effected, prove conclusively that hyperæmia of the spinal cord is at once the proximate cause and complete explanation of this hitherto mysterious phenomenon.

14. (g) *Abnormally High Temperature within the Rectum.*—The doctrine that the white substance expelled from the bowels, or found in them after death, is not the result of passive exudation, but is chiefly a product of excessive glandular activity, is in perfect accordance with, and for the first time explains, the fact, that during collapse the temperature within the rectum is extraordinarily high, and that it falls simultaneously with the emergence of the patient from that state. In a case of cholera, mentioned in the *Lancet* in 1832, the temperature in the rectum was 105°. "Late experiments in Paris," as remarked by Dr. Macpherson,\* "show that the temperature rises to about 103° in the rectum." In a case of collapse, "not in a severe form," under the care of Dr. Weber, "the temperature in the axilla on the second day of collapse was 97° Fahr., while that in the rectum was 100°·5; on the following day it was in the axilla 97°·6, in the rectum 99°·5; and, again, a day later, in the axilla 98°, in the rectum 99°·1. Dr. Weber remarked that he had also found, in several other instances in which he had examined for it, that during the collapse the temperature in the rectum was more or less above the average of health; that it had been in one case of severe collapse as high as 103°, while in the axilla at the same time it scarcely exceeded 95°; that with progressing recovery the temperature in the rectum had decreased, while that in the axilla had increased."† In interesting accordance with these facts, the temperature in the groin is found to be higher than it is in the axilla. (See the first section of the preceding chapter.)

15. (h) *Excessive Activity of the Mucous Membrane of the Bronchial Tubes, of the Gall Ducts and Gall Bladder, of the Pelves*

\* "Cholera in its Home," p. 53. † *Medical Times and Gazette*, Sept. 1, 1866.



*of the Kidneys, and of the Female Genital Organs.*—These mucous surfaces yield evidence, on post-mortem examination, that their functions have been increased to an intense degree, and in exactly the same way as is observable in the alimentary mucous membrane: the glands of the uterus exhibit enlargement, just as the intestinal glands do. The generalization already expressed respecting the mode of innervation of the alimentary mucous membrane is strictly applicable to the glandular surfaces now in question, and therefore, though in respect to some of them its applicability has not yet been demonstrated experimentally, we are justified in assuming, that as the phenomena in question are identical, so are their causes. By way of confirmation, however, I can state from experience that the activity of the bronchial and of the vaginal mucous membrane may be increased by heat, and restrained by cold applied along the lower segments of the spine.

16. (i) *Abdominal Gripings.*—The fact has been well established, that when muscles are but inadequately supplied with blood, their susceptibility to nervous excitement is increased. This I hold to be the condition of the muscular coat of the intestines in cholera. The fact is indisputable, that the arteries generally throughout the body, excepting the larger trunks, contain a remarkably small amount of blood; and there is good reason, as I shall hereafter show, for believing that the branches nourishing the muscular coat of the intestines are no exception to this rule. On the other hand, if it be established that the nervous centres, whether spinal or ganglionic, are hyperæmic, it follows that these circular muscles are being stimulated to an intense degree. The inevitable result is, vigorous contractions, and, owing to their expulsive efforts, more or less vehement movements. When these contractions occur with especial intensity at particular points of the intestinal tube, and especially if the progress of its contents be temporarily arrested at these points, abdominal gripings ensue. If the intestines contain wind, and be at the same time subjected to vigorous vermicular movements by the contractile force of the circular muscles, in the manner just explained, the air is compelled to traverse large tracts of the canal in rapid succession, and hence the distressing rumbling known as borborygmi.

17. (j) *Excessive Expulsive Activity of the Stomach and Bowels.*—This, for present purposes, is sufficiently explained in the preceding paragraph: for to the question of the exact *modus operandi* of vomiting, I need advert only to the extent of observing, that I have proved it to be producible by hyperæmia of the middle-third of the spinal cord, and that it can be allayed by annulling that hyperæmia by the application of ice along the same region.

18. (k) *Simultaneous Excessive Activity of the Thoracic and Abdominal Muscles.*—That this excessive activity is caused by an undue afflux of blood in the spinal cord is so well known, that I need only recall the knowledge of the fact in this connexion.

19. (l) *Sweat in all its Grades of Copiousness and Fluidity.*—In



the introduction I have given a brief outline of the physiology of perspiration, and have shown that it is identical with that of the secretion of mucus. I have also shown that the sweat glands are supplied with nerves from the cerebro-spinal axis, and that though they may be stimulated into extreme activity by external heat—as, for example, a tropical atmosphere or a Turkish bath—their functions are essentially under the control of the spinal centres, which, if anæmic, will permit the sweat glands to remain inactive, but which, if hyperæmic, will force them to elaborate and exude sweat, even though the surrounding temperature be very low, and the skin generally anæmic. These demonstrations afford a complete explanation of the immediate cause of the profuse sweating in cholera, while the fact itself is an additional and most convincing proof of the presence of an excessive amount of blood in the spinal cord. In the early stage of collapse, sweating is often profuse; and as there is still a considerable amount of blood in the system, its watery constituents are copiously effused through the skin, and thus the sweat is nearly of a normal character. But as the disease advances, the supply of blood to the skin becomes more and more completely cut off; the sweat glands, still stimulated even more energetically than before, secrete sweat of an increasingly concentrated character; and hence the oily or viscid nature of the fluid, with which the cold skin becomes at length bedewed.

20. (*m*) *Derangements of Sensibility*.—These are generally of the negative sort; but when they are of a positive character, they are of course due to abnormal affections of the cerebro-spinal axis.

21. (*n*) *Tremors*.—These, in respect to their physiology, are intimately allied to cramps, and require especially for their production, simultaneous and energetic action of both the negative motor and positive motor nerves distributed to the muscles; the first lessening their supply of blood, the second stimulating the enfeebled organs.

22. (*o*) *Muscular Twitchings*.—These depend, of course, on the special excitement of the motor nerves distributed to the affected muscles, their tendency to occur being increased as the muscles in question become anæmic.

23. (*p*) *Fixed, Stony Expression of the Face*.—When there is decisive hyperæmia of the medulla oblongata, but not sufficient to issue in convulsions, the facial muscles receive so much nervous stimulus beyond the normal amount, as causes them to assume a state of slight tonic spasm. Hence the muscular outlines become peculiarly distinct, and the expression of the face stony and immovable.

24. (*q*) *Tonic Hardness of Muscles generally*.—I observed this symptom in one of the patients treated by me in 1865 at Southampton, and I have seen it mentioned in print.\* It is merely an extension

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\* A case of this kind is reported in *The Medical Times and Gazette*, August 11, 1866.

to the muscles of the limbs, and it may be to those of the trunk, of that slight spasmodic condition which, as just mentioned, is commonly observable in the muscles of the face, and is due to a like cause, though of course proceeding from hyperæmia of the spinal cord, having an extensive range. That it is due to this cause is rendered certain, not only by all we know of the functional relation of the nervous to the muscular system, but, in the instance just mentioned, by the rapid disappearance of it (within five minutes) after ice had been applied along the spine.

25. (r) *Tightness across the Lower Part of the Chest.*—This symptom is probably partly referrible to the contraction of the bronchial tubes and of the pulmonary arteries, which impedes the passage of the blood through the lungs; but in so far as it is especially localized in the lower part of the chest, it is mainly due to tonic contractions of those respiratory muscles which co-operate in drawing the ribs downwards, and in diminishing the thoracic cavity. It is probable that the diaphragm is especially implicated in these tonic spasms. Of course they are wholly referrible to exalted functional activity of the spinal cord.

26. (s) *Cramps and Convulsions.*—The observations already made concerning the abnormal contractions of the intestinal muscles and the tonic contractions of the voluntary muscles are applicable to cramps, which are but tonic muscular contractions of an extraordinarily energetic character. When convulsions occur in cholera, the cerebral arteries have become contracted to such an extreme degree as wholly to deprive the brain of blood, and thus to abolish consciousness. The presiding and co-ordinating power over the functional activity of the spinal cord having been overthrown, the impulses of this organ, already abnormally and extraordinarily energetic, are aimless and chaotic; and hence those non-co-ordinated and disorderly muscular contractions called convulsions, which, when occurring in cholera, are generally induced by the crowd of intense impressions transmitted to the spinal cord from the struggling lungs, and are a final effort of nature to expand the tightly-bound chest, bronchial tubes, and pulmonary arteries, and thus to permit of the aeration and circulation of the imprisoned blood.

27. (t) *Restlessness and Tossing of the Limbs to and fro,* are proximately due in part, no doubt, to the difficulty of breathing, but mainly, I believe, directly to the excessive hyperæmia of the spinal cord, which, while still dominated by the brain, expresses its functional energy in these aimless movements.

28. (u) *Extreme Contraction of the Urinary Bladder.*—The muscular coat of this organ is under the governance of the spinal cord, which is effecting energetic contraction of every muscle under its control, where the will is unable to exert a prohibitive power, or where no other form of resistance is present. Now, during collapse, the function of the kidneys is completely arrested; therefore no urine passes from them into the bladder; and thus, while no

distension of the bladder is effected, no obstacle to its extreme contraction, under the vehement nervous stimulation to which it is subject, exists.

#### NEGATIVE PHENOMENA.

Every negative phenomenon denotes absence of some function or condition which exists as an attribute of the healthy organism, and is alike immediately due to the absence of arterial blood. It may be convenient to denote the state of collapse alone by the term *algide*; but, in doing so, we are apt to mislead ourselves, for, in fact, there is no line of demarcation between the stage of invasion and that of profound collapse at which it can be truly said that the *algide* state first manifests itself. This state is one comprising all degrees of intensity, and they merge into each other so imperceptibly, that it is impossible to mark the first advent of it, or to define its several degrees. During the stage of invasion, even before diarrhœa sets in, the temperature of the body begins to decline, and, as the disease progresses, continues to fall. Sometimes one part of the body, and sometimes another, becomes, at the onset of the disease, notably cooler than the rest. This local fall of temperature is more especially notable in the lower extremities, and over the surface of the abdomen. In the first part of this work I have demonstrated that the so-called premonitory diarrhœa of cholera is due to hyperæmia of the spinal and sympathetic nervous centres, and have given evidence that when that hyperæmia is subdued the diarrhœa ceases, and the surface of the body, which had been notably cool, becomes warm. When the nervous hyperæmia, which, in its slight degree, is productive merely of premonitory diarrhœa, with its accompanying symptoms, continues and increases, the contraction of the systemic and pulmonary arteries, as well as of the bronchial tubes, occasioned by the energetic action of the sympathetic ganglia, becomes increasingly stronger and persistent. The supply of blood to every part of the body, excepting the nervous centres and those organs which, by virtue of overpowering stimuli from the positive motor nerves, continue to attract it with vehement energy, is gradually lessened, and at length more or less completely cut off. During the increase, step by step, of the force and energy with which the muscular coats of the systemic and pulmonary arteries, as well as of the bronchial tubes, contract on their contents, the various phenomena of cholera which I call negative, are produced, and their progressive development denotes the intensity with which their proximate cause is operating. I shall now pass these symptoms in review, explaining them as I proceed.

29. (a) *Slight Headache*.—It may be stated as a general law, that the affinity between arterial blood and the nervous centres is greater by far than that which exists between it and any other of the organic structures; and, as a matter of fact, they are, in proportion to their bulk, supplied more copiously than any other, except perhaps



the glandular, organs. It is obvious that the subtle atmospheric agents, whatever they may be, which induce the sudden development of epidemic cholera, heighten to an extreme degree the already supremely intense affinity in question. Thus it seems as if the blood withdrew itself to the citadels of life—the nervous centres—which, in fatal cases, are, of the various parts of the body, the last to die. But though the brain retains its circulation, and performs its functions so healthily as to excite general astonishment, while the life of nearly all the other organs of the body is rapidly or suddenly declining, the relation of the ganglia of the cervical sympathetic to the brain, involves the necessity, when they are hyperæmic, of the exertion of a considerable amount of contractile force by the muscular coats of the cerebral arteries. Hence it is that though the attraction between the nerve cells of the brain and arterial blood is so potent as to cause it to flow to them through the cerebral arteries, and thus to keep them dilated, notwithstanding the constringing force which is exerted upon them, that force does actually result in lessening the normal supply of blood to the brain, and thus produces the several cerebral symptoms about to be mentioned, of which slight headache is one. Observers of disorders of the nervous system are well aware that headaches of various degrees of intensity are produced by cerebral anæmia, as well as by cerebral plethora.

30. (*b*) *Deafness of various grades*, which is a very common symptom of cholera, is due to inadequacy in the supply of blood to the sensory ganglia.

31. (*c*) *Tinnitus Aurium*, or singing in the ears, is due to the same cause, and is an instance of that excessive excitability often observable in the brain of anæmic patients.

32. (*d*) *Dizziness* and *Slight Faintness*, or *Syncope*, I need hardly say, are symptoms of partial anæmia of the brain.

33. (*e*) *Drowsiness and Sleeplessness*.—It has been demonstrated by Mr. Durham (see Guy's Hospital Reports) that normal sleep is coincident with a supply of blood to the brain less than that which it receives during normal wakefulness. I have been able, by influencing the circulation in the sympathetic ganglia, through modifying their temperature, to confirm his conclusions completely. But I have also established, to my own satisfaction at least, that if the amount of blood in the brain be a certain degree less than that coincident with normal sleep, sleeplessness, and sometimes low delirium, as in delirium tremens, ensues. If the supply of blood be restricted still further, morbid drowsiness, or, finally, coma, is induced. Irrespective of these states, which are phases in the progress of cholera, there is no doubt that sleeplessness, due to the distress and anxiety occasioned by the disease, often prevails.

34. (*f*) *Mental States*.—These, as already mentioned, are very striking, and their proximate cause will be fully understood from the foregoing explanatory observations. I may, however, point out

how completely they elucidate the fact that, though the force and activity of the intellect may be impaired, its functions are, as a general rule, never perverted until the stage of collapse has passed, and that of reaction has set in. *Anxiety*, often also a prominent symptom, is usually coincident with moderate depression of the cerebral circulation, which, if still further impeded, results in *apathy* and *indifference*, also notable accompaniments of cholera.

35. (*g*) *Non-secretion of Tears, Saliva, Bile, and Urine*.—Dr. Parkes, when speaking of what I maintain to be the copious *secretions* of sweat and mucus which occur in cholera, says, “These processes occur, not as secretions, but as exhalations; that is to say, the exuded fluids of the blood are not subjected to the vital influences of cells; true secretion seems arrested; no urine is formed, no saliva, and no tears; the bile and pancreatic juice are not voided, and probably are not formed.”\* What I have said in the introduction concerning secretion, and, in this chapter, concerning the activity of the alimentary mucous membrane, is, it will be seen, in direct opposition to Dr. Parkes’s opinion, which, in the passage just quoted, he endeavours to strengthen by showing that the lachrymal, salivary, hepatic, pancreatic, and renal glands cease to act. The arrest of the function of these glands affords, however, no conclusive evidence that the glands of the mucous membrane, and of the skin, cease to secrete. As I have shown, every gland and secreting surface is supplied, in addition to the usual sensory nerves, with two motor nerves, one from the cerebro-spinal, the other from the sympathetic system: the former, or the positive, being the one which endows the gland cells with power to attract blood to themselves, and to select from it their peculiar products; the other, or the negative, being the one which regulates, or rather checks, the supply of blood to the gland by constricting the glandular arteries to which it is distributed. Its operation is analogous to that of the governor of a steam-engine, which, in proportion as its revolutions become increasingly rapid, lessens the supply of steam to the cylinder: in like manner, when the gland cells are stimulated to make copious draughts on the blood, the gland arteries are subject to a correspondingly energetic contractile force; for, as a general rule, the relation between the spinal and ganglionic nerve-centres controlling any particular gland is such that, when one becomes hyperæmic, the other does also, and hence an excessive stimulus is sent to the gland from both the positive and negative motor nerves simultaneously. Now, by altering the weight of the two balls at the end of the iron rods of the governor of the steam-engine, it is obvious that they may be made to rise, more or less rapidly, by the centrifugal force which becomes operative when the instrument revolves; and thus, by the adjustment of valves in

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\* “Researches into the Pathology and Treatment of the Asiatic or Algid Cholera,” pp. 107, 109.



connexion with the rods, the supply of steam to the cylinder can be lessened whenever the revolutions of the governor attain any given rapidity. In like manner, the relative proportions in which the nervous energy is transmitted by the positive and negative motor nerves, respectively, to the gland, will determine whether the power of the cells to attract blood, or the power of the contractile coat of the arteries to cut off the supply, shall predominate; or, again, whether the two forces shall balance each other. If the power supplied by the positive motor nerve predominates, the gland will continue to secrete, the blood being drawn through the artery, notwithstanding the constricting force exerted upon it; but if the negative motor nerve predominates, the gland, though in a healthy condition and capable of performing its functions, will cease to do so, because the glandular arteries contract with overpowering energy, and wholly cut off the supply of blood.

Now, the positive motor nerve of the lachrymal gland is a filament from either the third or fourth cerebral nerve, that of the parotid consists of a branch, or branches, of the auriculo-temporal, and that of the sub-maxillary gland is the chorda-tympani. Thus the positive motor nerves of all these glands have a cerebral origin. We have already seen that though the cerebral functions continue during collapse, the brain is more or less anæmic, consequently it is unable to transmit even normally energetic impulses through the positive motor nerves to these glands. On the other hand, the sympathetic ganglia, from which the negative motor nerves to these glands emerge, are still hyperæmic. It seems, therefore, inevitable that, during choleraic collapse, the secretion of tears and of saliva should be arrested, although the glands of the skin and of the mucous membrane are in a state of extreme activity. Why the relative strength of the positive and negative nervous currents to the liver and the kidneys should be such as to insure that, when there is excessive hyperæmia of the spinal and ganglionic nervous centres, the negative motor nerve-force should predominate, is not obvious: the reason may hereafter become apparent; but indeed there are, in different cases, so severe as to prove fatal, considerable differences in respect to the completeness with which the hepatic secretion is arrested: in most cases its presence in small quantities in the evacuations can be detected by chemical tests; and it appears that all the phenomena of collapse of a profound character may be present, while the flow of bile nevertheless continues. In a fatal case, under the care of Dr. Olliver, of Southampton, this year (1866), the particulars of which he kindly communicated to me, a girl was attacked with purging at 2 a.m. of July 9th, vomiting soon after ensued, and the evacuations, until they ceased altogether, consisted of a bright yellow fluid. At 6 p.m. she was cold, pulseless, voiceless; in short, in complete collapse, and died the following morning. Mr. Darwin's doctrine of "natural selection" affords, I apprehend, the true explanation why the secretion of milk continues during collapse; for obviously only those

human tribes whose women could continue to give suck throughout periods of great disturbance of the nervous and vascular systems could continue to exist.

I am not aware that any record has ever been published of the post-mortem condition of the lachrymal and salivary glands in cases of cholera; but the post-mortem state of the liver and of the kidneys is perfectly accordant with the requirements of the theory here expounded, and is alone satisfactorily explicable by it. The volume of the liver is diminished, it is paler than natural, bloodless, and exhibiting no evidence of disease, or the action of a poison; its secreting structure, when examined by the microscope, is found to be normal. If the reader will refer to section 15 of Chapter II. he will see that the only abnormal condition of the kidneys is venous congestion, to which I shall soon advert. If the pancreatic secretion is also arrested, as Dr. Parkes affirms, though I am not aware of the existence of any decisive proof of the fact, the remarks just made, respecting the other special glands, apply equally to it. I may here observe that if the arrest of the functions of these glands were due to the operation of a poison, its influence would be likely to appear, not only in the post-mortem condition of the glands, but also in some impairment, at least, of their functional power, when the patient emerges from the state of collapse; but it is notorious that both bile and urine, of a healthy character, are forthwith usually secreted. When albumen appears in the urine after collapse, it is due simply to the excess of the vascular reaction from the intense spasm of the renal arteries.

That suppression of urine is, at least, frequently caused by spasm of the renal arteries, is well illustrated by the results of vivisectional experiments on the kidneys made by Bernard, who found that if a large opening were made in the abdomen, and the intestines were pushed aside, in order to expose the kidneys, a suppression of urine was almost always the consequence. He says (I translate literally), "An operation thus severe induces almost always in the case of the dog or rabbit, if not immediately, at least after a very few seconds, suppression of urine, and then the blood in the renal veins is seen to assume a dark colour, and often to become as black as that of the inferior vena-cava." He further remarks, in a note to this passage, "In man, pain and mental emotions are also capable of arresting the secretion of urine. M. Jobert de Lamballe, in his '*Plastic Surgery*,' has mentioned cases of the operation for vesico-vaginal fistula, in which, in consequence of emotion, the flow of urine was suspended during the whole time of the operation, and, in some cases, for a considerable time afterwards."\*

36. (*h*) *Short, Struggling, and Rapid Respiration.*—The energetic

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\* "Sur les variations de couleur dans le sang veineux des organes glandulaires suivant leur état de fonction ou de repos." Par M. Claude Bernard.

"Comptes Rendus des séances de l'Académie des Sciences." 25 Janvier, 1858.



action of the sympathetic nerve results in the contraction of all the muscular tissue subject to its control, and constriction of the muscular coat of the bronchial tubes, as well as of systemic and pulmonary arteries. Hence it is, according to the hypothesis here expounded, that while on the one hand the contractions of the pulmonary arteries impede the access of their contents to the pulmonary capillaries, the contractions of the bronchial tubes impede the access of air to the air cells. This doctrine harmonises perfectly with the pulmonary symptoms of the disease, and is the only one which completely explains them. When the lungs are examined after death, they are generally found to be extremely destitute of blood, both the capillaries and the smaller branches of the pulmonary arteries being empty, the blood being restricted to the larger tubes. Referring to the absence of air in, and the consequent collapse of, the lungs during life, Professor Parkes observes, "It appears that this shrinking can, during life, be overcome by the voluntary efforts of the patient, or by artificial inflation, without beneficial effect on the disease, and that it is therefore merely a consequence. *Is it possible that the muscular contractility of the tubes could be increased?*" This suggestive question, which I have put in italics, shows how near to the truth, as I conceive it, Dr. Parkes had reached, in respect to the point in question. This correct surmise, had he not been already wedded to the doctrine of a blood poison, would probably have led him to anticipate all which it is the object of this book to expound. His statement that the shrinking of the lung can be overcome by the voluntary efforts of the patient, is confirmed by a fact which I witnessed, and which is mentioned in the record of Case 1 (Chap. VI.); but in this case, though there was already considerable lividity of the hands and face, the vehement efforts of the patient to inspire deeply were attended with positive benefit, and the hands and face became less dark. This fact of the power of an increased supply of oxygen in the lungs, to draw an additional quantity of blood through the constricted pulmonary arteries, is analogous to the cases already mentioned in which the positive motor nerves of glands draw blood to the gland cells, notwithstanding the constricting force to which their arteries are subject. The possibility of inflating the lung, partly at least during life, and completely after death, proves that there is no organic obstruction in, or injury of, the organs, while the immediate arterialisation of the blood, demonstrated by its change of colour which follows the inflation, certainly after death, and in the one case just mentioned by me, during life, proves that the derangement in the pulmonary tissue is only a functional or dynamic one, and that the blood itself, whatever other changes may be alleged to have occurred in it, is susceptible of healthy arterialisation. These considerations must, I think, constrain the admission, that all the pulmonary symptoms in question are completely explicable by the hypothesis, that excessive hyperæmia of the sympathetic ganglia presiding over the pulmonary

artery and the bronchial tubes, induces in their muscular coats extreme and persistent tonic spasms, which at once cut off the access of venous blood to the pulmonary capillaries, and of air to the air cells.

37. (*i*) *Cold Breath*.—This very characteristic and universally recognised symptom is an inevitable result of the conditions just explained. The amount of oxydation throughout the body, the lungs themselves included, being very slight, the temperature of the small amount of blood passing through the lungs is extremely low; and, of course, the very small quantity of carbonic acid and watery vapour which is exhaled, being comparatively cool, is able to raise the temperature of the residual air in the lungs, and of that which is expired, very slightly indeed. [Moreover, through the predominant action of the positive motor nerves, the secretive action of the bronchial mucous membrane is increased in the same manner as is that of the alimentary mucous membrane, though not to the same degree. Evidence of this increase is often found in the form of a considerable amount of frothy mucus, and generally in the form of a white adhesive substance in the smaller tubes; this substance, like to that found in the alimentary mucous membrane, consists, as I believe there is no doubt, of exfoliated epithelium mixed with mucus. Now, assuming an increase of bronchial mucus, the exhalation from the lungs, contained in the expired air, will have its watery elements proportionately increased, and the more vapour the cold expired air contains, the more notable and striking will its coldness become.]

38. (*j*) *Enfeeblement of the Voice; Aphonia*.—The impairment, or extinction, of the vocal power is simply a result, of course, of the contraction of the bronchial tubes, by which the free admission of air into the lungs is prevented. The patient, therefore, has the command of too small a supply of it to permit of its expiration with force enough to produce adequate vibrations of the vocal cords.

39. (*k*) *Oppression and Burning Pain at the Præcordia, and Left Epigastric Region*.—These distressing symptoms are probably due to, at least, three abnormal states induced in these regions; *first*, to the muscular contractions effecting a tightness across the lower part of the chest, as already explained; *second*, to the extreme distension of the right heart, and the main trunk and branches of the pulmonary artery; *third*, to distension of the spleen. It is the latter only to which I shall advert in this paragraph. In intermittent fever, the spleen, as is well known, becomes enlarged, and extremely so if the disease attacks the patient many times. In fact, as a consequence of this disease, rupture of the spleen sometimes occurs. The first distension of the organ is, I believe, coincident with the first onset of the cold stage, and the oftener this stage recurs, the larger the spleen becomes. Now, the remarkable way in which the spleen

is thus affected during the course of ague, is, in my opinion, an instructive illustration of what occurs in the same organ during the collapse of cholera, as a result of the energetic action of the negative motor nerves. By constricting the systemic arteries throughout the body, and thus emptying, or nearly emptying, all but the larger trunks, they force the blood in every possible direction in which it encounters least resistance; the spleen is thus made the receptacle of an enormously large quantity, which distends it extremely, and produces the great pain, as well as the sense of burning heat, complained of in the left side. It seems to me probable that the considerable heat of this mass of blood close to the stomach, which is itself the seat of intense glandular activity, and therefore hotter than natural, affords the explanation why *cold* drinks and ice are so peculiarly grateful to patients in collapse. The post-mortem condition of the spleen of patients dying in collapse, differs, as already stated, in different patients. By referring to paragraph 14 of the previous chapter, the reader will, however, find evidence that the spleen is often observed after death to be enlarged, and to present internally vestiges of extreme congestion. Though it is also often found small, this fact does not disprove the hypothesis that it was congested and distended before death; for considerations hereafter adduced justify the belief that, in those cases in which the spleen is found of normal size, there has been a reflux of blood from it at the time of, and after death, produced as will be described in the paragraph in which I shall account for the elevation of temperature after death. Referring to a report of seventy cases examined by Virchow, Dr. Gull says, "During life he (Virchow) had repeatedly, by percussion, been able to determine that it [the spleen] was decidedly enlarged; and, in some cases, patients had complained of pain in the left side. In old persons no enlargement was ever observed; but in almost all other cases the spleen was so flaccid and shrivelled, that one was obliged to admit a previous enlargement, or rather so recent and rapid a reduction of volume that the capsule had not yet accommodated itself. The change of colour in the tissue, from altered blood, led him to infer a previous hyperæmia."

40. (l) *Algide Symptoms: Progressive Changes in the Visage, and in the Colour, Temperature, and General Aspect of the Skin.*—Among the earliest symptoms of the stage of invasion, paleness and coolness of the skin are remarkable; gradually, or with more or less rapidity and suddenness, the paleness merges successively into pallor, duski-ness, lividity, and, especially in the extremities, into dark cyanosis. Simultaneously the coolness increases, until it becomes "a mortal coldness," and the previously well-filled skin shrinks, and becomes corrugated, the fingers appearing in this respect as if they had been soaked for a long time in hot water. This group of symptoms denotes a diminution or arrest of the supply of blood to the surface of the body, and of those chemical transformations of the organic

textures which constitute nutrition and local life. Now this diminution or arrest of the supply of blood, can only be effected with the force or suddenness with which it takes place by spasmodic contractions of the circular muscles which surround the terminal arteries conveying blood to the surface of the body; for, as I have shown, no form of asphyxia or arrest of the pulmonary circulation, is reconcilable with the phenomena of cholera. When the arteries are thus spasmodically closed, the arterial blood is shut off from the surface of the body, and therefore can no longer impart to it its wonted ruddiness, or healthy hue; while, on the other hand, as arterial blood ceases to pass through the capillaries into the veins, and to cause the blood in the venous radicles to "move on," venous congestion in those radicles occurs, and imparts to the skin that dark blue and remarkable livid colour which cholera patients present. The only part of the human body in which a morbid process closely resembling this can be actually observed is the eye.\* In cases of idiopathic or cerebral amaurosis, when the retina and optic nerve are being wasted, the supply of blood is shut off; the retinal arteries shrink to the diameter of a hair, or become invisible, but the blood in the retinal veins, which are still turgid, remains stationary. That this state of the blood-vessels in the eye, and the consequent blindness, are caused by prolonged spasms of the arteries,—spasms originating in the same way as those pervading the whole body in cholera, and capable of being overcome in the same way as the cholera spasms may be, I entertain no doubt. Indeed, the truth of this doctrine has already been proved by the successful treatment of this form of amaurosis, by means of ice along the upper part of the spine. As the evolution of animal heat is a condition resulting from those chemical transformations of the organic textures which constitute nutrition and local life, and continuing only while they continue, their cessation necessarily implies the rapid fall of temperature observable in cholera cases. Of course the more isolated any part of the body is, or the more of surface in proportion to its bulk it presents, the more rapidly it will become cold; hence it is that the ears, lips, and extremities, become the coldest. On the other hand, the arterial blood being prevented from flowing into the small arteries, is mainly confined to the large ones, and, as already explained, within the spleen; but the excessively vascular tissue of the cerebro-spinal axis and of the sympathetic nervous centres continues to retain a supply of blood the longest of all. Thus it is that a considerable degree of warmth compared with the temperature of the rest of the body, is experienced not only in the epigastric region,

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\* The retinal veins of patients in collapse are perceptibly enlarged; but no diminution of the retinal arteries is observable: for the great nervous expanse of the retina continues to draw blood to itself as the brain does, although less abundantly than in health.

but over the region of the heart, and great blood-vessels, and in the forehead.

Assuming as I do, for reasons suggested in the introduction, that all the textures of the body are supplied with positive motor nerves, the action of which is the immediate cause of the attraction exerted by the constituent elements of each structure for arterial blood, I recognise in the phenomena just mentioned, evidence that the force of these nerves is far less than that of the negative motors which, by their predominant action, cut off the blood supply in the manner indicated.

The shrunken, or corrugated aspect of the skin, is also mainly due to the empty state of the arteries. It is probable, however, that this feature may be rendered more extreme than it otherwise would be by the copious drains on the blood, which are effected by the stomach and bowels. The general shrinking of the visage is an expression of the arterial contractions in question, but the peculiar cholera aspect is due to the combined operation of this cause, and that already explained, viz., the tonic spasms of the facial muscles. No feature in the cholera visage is more striking than that of the deep sinking of the eyes into their sockets, and when considered in connexion with the fact, that a free supply of blood is continued to the eye itself, is perhaps one of the most striking proofs adducible of the truth of my hypothesis, that the phenomena discussed in this section result from spasmodic contraction of the systemic arteries caused by hyperæmia of the sympathetic nervous centres. With the exception of a few branches ascending into the orbit from the infra-orbital artery derived from the internal maxillary, the structures within the orbit are supplied with blood by the same artery, the ophthalmic, which supplies the eye-ball itself. Now, if the diminution of arterial blood to the various parts of the body were due only to the fact, as alleged by Dr. George Johnson, that the passage of the blood through the lungs is impeded, the supply of blood to the eye-ball, and the structures extraneous to it contained within the orbit, should be equally and simultaneously arrested; but, as a matter of fact, while the ocular muscles, lachrymal gland, and adipose cellular tissue of the orbit, are deprived of blood, the latter so extremely so that it shrinks to the great extent, causing the eye to sink within its socket to the astonishing degree observable in cholera, the eye itself is so adequately supplied with blood until the near approach of death, as to enable complete visual power to continue. I explain the facts in question, by asserting that the predominance of the positive over the negative nervous energy, distributed to the arteries of the eye-ball and optic nerve, is much greater than obtains in respect to those which supply blood to the structures of the orbit extraneous to the eye itself.

41. (m) *Loss of Cutaneous Sensibility*.—This is simply due to the fact that the cutaneous nerves are deprived of arterial blood: their

oxydation, which is a condition of their functional activity, is therefore arrested. In cases of hemiplegia, when the circulation of one side of the face is impaired, there is often great anæsthesia. In these cases, normal sensibility may be very often speedily restored by the application of ice along the upper third of the spine. The remedial influence exerted, consists, as I explain it, in an increase of the circulation in the face, effected by depressing the undue energy of the irritated sympathetic ganglia.

42. (n) *Serous Exudation into the Intestines*.—The group of algid symptoms so pre-eminently observable in the skin, are present, and are produced in the same way, though in different degrees, if my hypothesis be correct, in every part of the body. But in the intestines special conditions obtain: as already explained, the positive motor nerves of the alimentary mucous membrane and intestinal glands being in extremely predominant and energetic action, a large amount of blood is drawn by them through the intestinal arteries, is passed through the glandular capillaries, and is forced into the intestinal veins. But while the venous blood generally is hindered from passing freely through the lungs, by contraction of the pulmonary arteries and bronchial tubes, that in the intestinal veins is arrested, simultaneously with the secretion of bile, by the intensely energetic action of the hepatic negative motor nerves, which, bringing the functions of the liver to a stand, prevent the passage of any blood through it. Thus, while on the one hand the excessive secretive activity of the intestinal mucous membrane and glands is causing an excessive amount of blood to be compressed by the agency of their capillary force into the intestinal veins; on the other, the blood so forced into them having no normal exit, first distends them to the utmost, and then the serous, or most fluid part of it, oozes through them—their extremely thin walls being rendered still thinner, and therefore extraordinarily permeable, by the distension to which they are subject. Hence the exudation of the enormous amount of serous fluid which is discharged by cholera patients, which, as well as the œdema of the intestinal walls and the serous infiltration beneath the epithelial sheaths of the villi contributing to their exfoliation, is not only accounted for, but shown to be absolutely inevitable.

43. (o) *Epithelial Exfoliation of the Intestinal Villi*.—That these villi are often denuded, numerous observers testify. In cases when they are not already bare, their epithelial sheaths are frequently found partially detached from them. Dr. Beale attaches immense pathological importance to this denudation. "It is probable," he says, "that the extent of this process of denudation, determines the severity or mildness of the attack. If the great majority of the villi have suffered, it is scarcely reasonable to consider recovery more probable than it would be after a very extensive burn or scald."\*

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\* "Microscopical Researches on the Cholera," *Medical Times and Gazette*, August 4th, 1866.

In my opinion, this exfoliation is exactly analogous to cutaneous desquamation, and is due to two causes, one of which, amongst others, Dr. Beale has himself suggested, viz., "the almost complete cessation of the circulation in the capillaries beneath." The other cause is the infiltration between the villi and their epithelial coverings of the watery part of the blood, from the distended venous radicles (see preceding section of this Chapter). If the arteries generally are in a state of spasm, only those which have peculiarly powerful draughts made upon them, viz., those which supply nervous tissue, and the glands of the alimentary canal, have full streams of blood flowing through them. Hence the intestinal villi are among the parts from which the supply of blood is cut off; and hence it is, that the columnar epithelial cells seem, according to the report of Dr. Beale,\* to be stunted, and in many instances the nuclei are much smaller than in health. In the intervals between their attached extremities, one fails to find those smaller and younger cells which, in the healthy state, gradually grow up to take the place of those cells which are removed, and give origin to new cells which, in their turn, become developed. So also it is to be observed, that the masses of germinal matter, so numerous near the surface of the healthy villi, are almost absent in many of these cholera cases."†

If the explanations expressed in this section and in section 13 of this Chapter, be correct, the epithelial cells found in the evacuations, and in the intestines after death, are derived from two diametrically opposite sources: one part, and by far the greater part, is the result of exuberant development—excess of life throughout the whole glandular system of the alimentary tract; the other part is the result of disintegration of the outermost and dying texture of the villi, which, being deprived of their blood supply, are themselves on the way to death. It is probable, however, that their denudation takes place in its maximum degree, as Dr. Parkes‡ believes, only after systemic death has occurred. This conclusion is justified by the observations of Dr. Andrew Clark, which I restate here:—"Patches of conoidal epithelium were never observed in the bowel discharge of any choleraic during life. After death such patches

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\* *Medical Times and Gazette*, August 18th, 1866.

† Dr. Beale thinks the condition of the cells described in the text "exhibits evidence of chronic change," and that "in most of the cholera victims important morbid alterations have been going on for months, and in some instances for years before death"! It appears to me that the observed condition of the cells in question is quite adequately and much more simply accounted for, as stated in the text, while the doctrine of "chronic change," is wholly irreconcilable with the two following notorious facts: *first*, many persons who have been in the enjoyment of perfect digestive and assimilative power and robust health, until the date of attack, are suddenly struck down by cholera; and, *second*, precisely in those cases in which extreme disorder of the alimentary canal, denoted by vomiting and purging, predominates over the spasmodic or algid symptoms there is the greatest chance of recovery.

‡ See his letter in the *Medical Times and Gazette*, August 25, 1866.



were found abundantly in the contents of the bowels." But this exfoliation of the villi, whenever, and to whatever extent it occurs, is merely one of many consequences and expressions of that general anæmic spasm with which the arteries are affected, and which is itself due to hyperæmia of the sympathetic nervous system. It is therefore, as it seems to me, as erroneous as it is unphilosophical, to consider this one among many results of a general cause, as having any special pathological significance, or as determining, in any degree (according to the extent of the process of denudation), "the severity or mildness of the attack."

44. (p) *Enfeeblement and Death of the Voluntary Muscles.*—The arteries supplying the muscles generally, being abnormally and powerfully constricted in the same way as all other arteries are, the nutrition of the muscular structure is gradually impaired, and at length, in fatal cases, completely arrested; and thus it is, that in the last stage of collapse, the patient, while possessing consciousness and considerable intellectual capacity, including the power of hearing and understanding what is going on around him, may yet be unable to give any sign that such is the case. Of course the temperature of the muscular system declines simultaneously with its nutrition. This is strikingly exhibited by the tongue, which is cool, cold, or, as is often said, "icy cold," corresponding with the successive degrees of arrest of the supply of blood to it.

45. (q) *Enfeeblement and Death of the Involuntary Muscles; and Consequent Cessation of the Discharges, though Secretion continues.*—This takes place by the same means, and in the same way as just explained in respect to the voluntary muscles. The first remarkable sign of decisive decline of vitality in the involuntary muscles, in cases of profound collapse, consists in the cessation of vomiting and purging. The vermicular contractions of the alimentary canal becoming too weak to eject the evacuations with the remarkable force characteristic of the stage of development of the disease, are now only able to expel the fluids so feebly, that they seem rather to ooze than to be ejected from the body; and at length they cease altogether, although there is still an abundance of fluid effused into the intestinal canal. It is probable, however, that simultaneously with the decline of vitality in both systems of muscles, the energy of the nervous centres may be already so far impaired, that the nervous arcs related to the muscles have all but lost their functional power. The continuance of the secretive action of the intestinal mucous membrane, after the tube has lost its expulsive power, is evidence, however, that the spinal cord is capable of continuing to effect chemical changes through the agency of the positive motor nerves after the muscles of the intestines have ceased to act.

The evidence in support of the hypothesis I am contending for presented by the foregoing explanatory review of the phenomena resulting from the predominant action of the positive and negative

motor nerves respectively throughout the system, will, I believe, be recognised as of a very conclusive nature. There is, however, in addition, a crowd of facts, which, when attentively considered, will be seen to constitute proofs no less indubitable and irresistible of the truth of the doctrine in question. I proceed to advert to some of the more important of them.

46. *Death of the Nervous System.*—I have already mentioned the fact, on the authority of Professor Parkes, that “two or three hours before death there is often some return of heat in the scalp and forehead, over the region of the heart or whole chest, and it may be also over the abdomen; the extremities are still icy cold, and the cholera visage is unaltered. This partial return of heat on the head and trunk is an immediate forerunner of death, and, as far as I have seen, is invariably a fatal sign; it is occasionally confined altogether to the cardiac region, and is sometimes astonishingly great.”\* This remarkable phenomenon is immediately due to the relaxation of the various branches of the pulmonary artery, of the bronchial tubes, and of those systemic arteries distributed over those parts of the body which during the disease have continued most vital, and have therefore preserved the highest temperature. The blood has continued to pass through these arteries most copiously, and is thus prepared to effect their dilatation, and to flow through them in fuller currents than before, the moment the energetic stimulus from the negative motor nerves, which has kept them in a state of tonic spasm, declines or ceases. Now already, when the phenomenon in question presents itself, the ganglionic nervous centres presiding over the arteries just mentioned have begun to die. Their convulsive grasp of the blood-vessels and air-tubes, which has already proved fatal to the system generally, is being relaxed, and they themselves are sharing the fate which, through their agency, has overtaken the entire organism. A last but vain effort for life is made, however, by the structures, released at length from the deadly influence of their excessive energy. The normal attraction between the venous blood in the pulmonary arteries and the air in the air-cells generates movement of the blood through the pulmonary capillaries; it reaches the left heart, and is thence forced most copiously into those systemic arteries just indicated, and thence, finding its way to the starving tissues, the usual vital changes occur. Meanwhile, as a result of the renewed oxydation occurring in the lungs, and in the parts supplied by the newly-relaxed systemic arteries, heat is evolved, and constitutes the phenomenon in question. But this local struggle for life is too late: its possibility depends on the presence of death in the nervous system, which soon seizing on the brain itself, closes the scene. This local increase of temperature before death is a strong

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\* “Researches into the Pathology and Treatment of the Asiatic or Algid Cholera,” pp. 101, 102.

proof that the blood-vessels are healthy; that the structures are healthy, and suffer only from lack of nourishment; that the blood itself is free from poison; and that the disease is seated in the nervous system.

47. *The Increase or Long Persistence of Heat in the Body after Death*, which is a well-established fact, is merely the continuance and extension over the whole body, after the death of the brain, of the series of actions commenced before death, and explained in the preceding paragraph. The whole sympathetic system having ceased its functions, the arteries throughout the body relax: the small supply of blood in them is drawn through the systemic capillaries; every particle of oxygen which it can yield up combines with the surrounding structures; and while there are elements to continue these changes, the temperature of the body is raised, maintained, or prevented from declining with the rapidity usual after death from almost all other diseases. The reflux of blood throughout the body denoted by the general increase or unusual persistence of its heat must inevitably result in lessening the bulk of any organ which may have been unduly distended with blood. Such an organ in choleraic collapse is the spleen, which is found unusually large during life, but which after death presents in respect to size no constant appearance. It seems to me probable that when the evacuations are extremely abundant, the spleen is less distended than in other cases, and that the great variations of size which it presents after death depend upon the amount of the evacuations, and the extent to which the post-mortem arterial relaxations result in a temporary renewal of textural vitality, which, of course, implies to the same extent a diffusion of the previously pent-up blood throughout the body.

48. *Post-mortem Changes in the Aspect of the Skin*.—These result from the chemical changes occurring, and caused as described in the preceding paragraph. The skin becomes lighter in colour, even when it has been especially dark, and has continued so for some time, because the oxydation of the blood changes it from blue to red, while the relaxation and partial dilatation of the terminal arteries cause the shrivelled aspect of the hands and feet to disappear.

49. *Post-mortem Muscular Contractions*.—These extraordinary, and, to the ignorant, terrifying phenomena have never, I believe, received a satisfactory explanation; but the hypothesis here expounded reveals the cause of them at once. The arteries supplying the muscles, were vehemently contracted before death, but are now relaxed; immediately before death the muscles were deprived of blood, and therefore enfeebled to the utmost degree; after death receiving a new supply of blood, their hungering constituent elements receive fresh nourishment, with it new strength, and, under the stimulus of the still hyperæmic spinal cord, which is the last to die, contract in the manner described. It seems to me that

if the waning life of the blood in the spinal cord be equally distributed, those apparently co-ordinated muscular movements, sometimes observable, are most likely to occur, and that when special segments of the cord retain their excitability longer than the rest, convulsive movements of one or of a few muscles only will result.

50. *The early Onset of Rigor-Mortis*, is a fact in extremely interesting accordance with the requirements of the hypothesis I am advocating. When the cervical sympathetic of an animal is divided, and one part of it excessively stimulated by means of galvanism, the arteries to which that part is distributed become, of course, strongly contracted, and in this manner a condition like to that which I affirm to exist in the arteries of a cholera patient in collapse is induced. Now, in such cases rigor-mortis invariably supervenes in the part of the animal thus experimented upon far more rapidly than elsewhere; and the fact that the same condition comes on with extraordinary rapidity after death from choleraic collapse, is a striking proof that the arteries of patients in that state are powerfully constricted by nervous agency.

51. *Additional Evidence from Post-mortem Appearances*.—The distribution of the blood generally is always in the veins. “Arborescent venous congestion” is the prevailing epithet used to describe the aspect of the vascular system, the arteries being empty. This is the condition which the hypothesis presupposes. Whatever may be the amount of hyperæmia of the spinal cord and ganglionic nervous centres during life, it by no means follows that they will be found in the same condition after death. Indeed, what has already been stated concerning the change in them after death, and its results in the arterial system, proves that their vascular state is modified when death occurs. It is, however, said that the spinal cord is found extremely congested, and that the sympathetic centres examined by Mayer were disorganized. These statements need confirmation. But if the sympathetic ganglia were disorganized, the fact would imply that their functional activity had been intensified to the extent of inducing destructive inflammation; and if so, this is the strongest fact which could be adduced in proof of the doctrine here propounded.

52. *Epidemic Attacks of Cramps and Spasms* occur, as it is well known, simultaneously with the presence of cholera as usually described, and are recognised, indeed, as partial manifestations of the disease. Dr. Gull mentions in his report, that “in several of the communications received, the writers state that, without any further symptoms, they suffered from frequent spasmodic pains in the bowels, and cramps in the calves of the legs, during the height of the epidemic, a circumstance also alluded to by Mr. Grainger in his report. ‘This peculiar twitching,’ he says, ‘was often observed in London during the late and previous epidemic, especially in those districts where the disease was severe. It is an interesting fact,

pathologically, that these slight cramps, which, like the other symptoms, were not, of course, always followed by collapse, being indeed most amenable to treatment, often occurred without being preceded, or accompanied, by diarrhoea, a circumstance which tends to show that the violent cramps and spasms accompanying the profuse discharges in collapse, do not depend on these, but rather on the morbid quality of the blood deranging the force of the spinal cord.' ”

53. I quite concur in the remark of Dr. Gull, that “the pathology of these irregular symptoms here given is doubtful, since they are equally referrible to a derangement of the nervous functions, from fatigue, and the other depressing concomitants of the epidemic.” But though Dr. Gull does not see in these facts a proof of the morbid quality of the blood deranging the force of the spinal cord, he will, I am sure, agree with Mr. Grainger, that whatever may be the state of the blood in the cord, “the violent cramps and spasms” are referrible to excessive excitability of that organ. And after Dr. Johnson’s reference of the spasms and cramps of cholera to the irritation of the muscles by the poisoned blood, it is satisfactory to find so high an authority as Mr. Grainger ascribing them to their true and only possible source. Inasmuch as these cases of *cholera spasmodica*, pure and simple, were most amenable to treatment, and were rapidly recovered from, it is inconceivable that they were produced by a zymotic poison, which, according to the recognised action of ferments, must end in affecting the entire blood, and therefore the whole system; but, on the other hand, if it be assumed that the proximate cause of cholera is a sort of excessive electric exaltation of the functions of the nervous centres, and that the resulting phenomena are, as already explained, of a dynamic character, the rationale of the spasmodic attacks in question becomes at once intelligible, and they are completely accounted for. This observation applies generally, not only to these attacks of cramps and spasms without vomiting or purging, but to all those forms of the disease which exhibit departures from the recognised type of the so-called “Asiatic” or algide cholera in respect to the absence of important characteristic symptoms, the intensity of the disease, and to the rapidity of its march. Referring to the question, how it happens that cholera presents itself in such various aspects, in such different degrees of intensity, and why the rapidity of its march differs so wonderfully in different cases, in different climates, and in different epidemics, I venture to express the belief that these differences can only be accounted for by the hypothesis here put forward. The following explanation of one of the most remarkable and hitherto unintelligible features of cholera will probably be held to justify this observation.

54. *Differences in the Relative Activity of the Positive and Negative Motor-Nerves in different Cases.*—The question whether the loss of fluid by vomiting and purging is the immediate cause of collapse, or

whether it exercises any influence in producing it, has been long agitated and is the subject of widely different opinions; but the most careful observers generally concur in the conclusion, that the relation between the loss of fluids and the state of collapse is not a causal one. Dr. Gull closes that part of his Report which relates to the pathology of collapse as follows: "Although in a large number of instances the intensity of the symptoms is in a general way proportionate to the amount of the effusion, yet that this will only in part explain the attendant collapse, which often appears to be in no inconsiderable degree due to the adynamic state of the ganglionic nervous system, induced either primarily by the poison, or secondarily by the lesions of the affected mucous surface." And Professor Parkes says: "My cases bear out the observations of Scot, Jameson, Orton, Kennedy, Copland, and, in fact, almost all the English writers of reputation, that there is absolutely no ratio between these two classes of symptoms; or that they appear even to observe an inverse ratio to each other."\* Dr. Johnson, who devotes a section of his book to the discussion of this question, concurs in the conclusion of the writers just mentioned. But no author I am acquainted with gives any real explanation why the relative intensity of the two groups of symptoms differs so widely as they do in different cases.

Every reader, however, who has fairly apprehended the doctrines expounded in the first section of the introduction to this work, and in the present chapter, will perceive at once why the relation between the two groups of symptoms *cannot* be a causal one, and why, in one case one group, and in another the other, predominates. When the spinal cord is more excessively hyperæmic than are the sympathetic ganglia, the action of the positive motor nerves relatively to that of the negative motor nerves will be proportionately more energetic; and in such cases the group of symptoms which I have classed as *Positive Phenomena*, and consisting of vomiting, purging, sweating, cramps, fixed expression of the face, and contractions of muscles generally, excepting those of the blood-vessels and bronchial tubes, will predominate. On the other hand, if the sympathetic ganglia are more excessively hyperæmic than the spinal cord, the action of the negative motor nerves relatively to that of the positive motor nerves will be proportionately more energetic; and in such cases the group of symptoms which I have classed as *Negative Phenomena*, and which in their extreme development constitute the algide state, or collapse, will predominate.

If it were possible for cases to occur in which the positive motor nerves were alone operative, the consequent purging, vomiting, and cramps might persist an indefinitely long time; and when they at length resulted in death, they would do so from sheer exhaustion of the blood from the body. On the other hand, if it were pos-

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\* Op. cit., pp. 79, 80.

sible for cases to occur in which the negative motor nerves were alone operative, the duration of the consequent spasms of the arteries throughout the body, and of the bronchial tubes, would wholly depend upon the intensity of those spasms: if they were slight, life might be indefinitely prolonged; if they were severe, it would be quickly terminated.

Though cholera never consists in the exclusive operation of either the positive or the negative motor nerves, sometimes the activity of the former, sometimes that of the latter, predominates extremely. In the first case, the result is, the form of the disease called *bilious cholera*, which is most prevalent in temperate climates; and which, as Sir Thomas Watson observes, "shows itself in this country more or less every autumn, and prevails extensively in some years as a minor epidemic." In the second case, the result is extremely sudden prostration; and not infrequently death ensues so rapidly, that though the characteristic cholera fluid is found on post-mortem examination to have been effused into the intestines, none is discharged during life. Cases of this kind, called *cholera sicca*, are the nearest approach observable to a typical form of cholera in which the negative motor nerves have an exclusive predominance. This form of cholera is by far the most appalling and the most destructive, and often presents itself in tropical climates, especially in India; it also occurs, however, though more rarely, in temperate zones, and is sometimes seen even in this country. "Authentic cases of cholera are on record by several of the most eminent writers on this subject, entirely divested of these symptoms [vomiting, purging, and cramps], and the suddenness with which the poison sometimes extinguishes life is extremely remarkable. When the cholera reached Muscat, instances are given in which only ten minutes elapsed from the first apparent seizure before life was extinct. Instances of the apparently rapid action of the cholera poison are related by Dr. Gavin Milroy, in a historical sketch of the epidemic of 1817, and at Kurrachee, in 1845-6, he relates, that 'within little more than five minutes hale and hearty men were seized, cramped, collapsed, and dead.' Instances of death taking place in two, three, four, or more hours, are extremely common. When the disease broke out at Teheran, in May, 1846, Dr. Milroy states that those who were attacked dropped suddenly down in a state of lethargy, and at the end of two or three hours expired, without any convulsions or vomitings, but from a complete stagnation of blood."\* An American writer, referring to my theory of the nature of cholera, implies that I ascribe the disease to "tetanus of the sympathetic." This phrase is a concise summary, however, of only half my doctrine, viz., that respecting the cause of what I call the negative phenomena;

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\* "The Science and Practice of Medicine." By W. Aitkin, M.D. Third edition, vol. i. p. 631.



but the impressions made by the disease on some observers referred to by Professor Parkes have suggested to them an analogy between cholera and tetanus. The fact that this comparison has been made is in interesting accordance with the pathology here explained, which at once shows the nature and extent of its justification. The more rapid the onset of the disease, or, in other words, the more vehement or tetanic the action of the negative motor nerves, the less time is there for evacuations to occur, and the more the phenomena present themselves in the likeness and with the suddenness of general spasm.

As spasms affecting every artery in the body, as well as the bronchial tubes, may come on with extreme rapidity, so they may pass off not less rapidly; and hence precisely in those cases of recovery in which the loss of fluids by vomiting and purging has been small, while the more formidable algide symptoms have seemed to be the heralds of death, is recovery most rapid, because the system, retaining the whole, or nearly the whole, of its sanguineous elements, possesses a restorative power of which patients who suffer from violent and prolonged vomiting and purging are deprived. "We can now understand," though by virtue of an explanation differing widely from that of Dr. Johnson, of whose adopted language I avail myself, "the sudden coming on of collapse, and its sudden passing off. Robust men falling down 'as if they had drunk the concentrated poison of the Upas-tree,' and recovering again almost as rapidly as 'patients who are resuscitated after submersion in water.'"<sup>\*</sup> We can also understand how it is that those patients who have suffered most from prolonged vomiting and purging, although they never approached death so nearly as the "robust men" just mentioned, are the longest in recovering. I may observe here that the hypothesis of a blood-poison as the proximate cause of cholera, while affording no explanation of the facts now discussed, is wholly at variance with them. Those patients in whom the so-called eliminative process has been least active recover the soonest; those in whom it has been most active linger the longest.

55. *The Stage of Reaction.*—When this is of the most favourable character, the accompanying symptoms are all mere results of the return of the nervous centres to a healthy condition. The muscular coats of the systemic and pulmonary arteries as well as of the bronchial tubes being no longer unduly stimulated gradually relax; air and blood are freely admitted to the lungs again; the blood in the large arterial trunks and in the portal vein is again enabled to permeate their smallest terminal branches; and thus textural nutrition in all parts of the body is revived, the functions of the liver and kidneys being simultaneously resumed. Meanwhile vomiting and purging gradually cease; the fluids still ejected from the stomach often become tinged with or contain a considerable amount of bile;

<sup>\*</sup> "Notes on Cholera: its Nature and Treatment," p. 54.

the stools have again a fæcal odour; urine, at first in small quantities, of a high colour, and often containing albumen, but soon of a normal character, is voided; the shrunken visage is quickly changed for its natural aspect; the whole skin becomes warm and suffused with blood, and the patient, often appearing as if he had suddenly gained flesh, recovers his wonted health with astounding rapidity.

56. Not unfrequently, however, the prolonged and vehement vasic spasms are followed by excessive relaxation—the excess being of various degrees, and greatest sometimes in the brain, sometimes in the lungs, sometimes in the alimentary canal, and sometimes in the kidneys. Mental torpor or delirium, pulmonary congestion or pneumonia, enteric disorders, albuminuria—one or all supervening according to the position and extent of the excessive vasic relaxation in question. When this excessive reactionary relaxation pervades the whole body, there is general fever, which may be sthenic or asthenic, and more or less typhoid, its special character being determined by constitutional predisposition, the amount of exhaustion which the patient has undergone in the previous stages, and not seldom by the abundance of medicines which have been administered, and which, remaining during collapse unabsorbed or inactive in the alimentary canal, are now exercising a baneful influence on the system. Special complications, sloughing of the cornea, for example, sometimes occur in this stage; but this, and indeed all the phenomena of reaction, proceed from the one common proximate cause above described; they are not peculiar to cholera, and possess no characteristic significance. Sometimes a slight discrete roseolous eruption during reaction is observable; and this phenomenon, dignified by the name *Roseola CholERICA*, is thought to confirm the doctrine that cholera is a zymotic disease. To me it appears simply as a slight capillary ecchymosis consequent on the excessive energy of the reaction, and identical in nature and origin with spots of like appearance often observable on epileptic patients.

57. *The Analogy between Cholera and Sea-sickness.*—In my pamphlet on sea-sickness\* (pages 26-7), I suggested the existence of an analogy between that malady and cholera, and predicted that the latter would be “both averted and cured by ice applied along the back.” Further study deepens my conviction of the existence of that analogy. Sea-sickness “is very often preceded by vertigo, and sometimes by headache. In some individuals, there is a feeling of uneasiness, sinking, and great distress at the epigastrium, which is described as worse than vomiting, and which occasionally lasts for a long time. Sickness of the stomach and vomiting, however, in general soon come on. The vomiting is frequent and exceedingly distressing, with convulsive heaving of the stomach, which extorts groans from the patient. Along with this is a general feeling of wretchedness and prostration, which, in bad cases, is sometimes so great as

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\* “Functional Diseases of the Stomach.” Part I. Sea-sickness: its Nature and Treatment. London: Trübner and Co. 1864.

to render the patient utterly inattentive to everything around him, and almost indifferent to life."\* The writer of this description certainly had no idea of any analogy between sea-sickness and cholera; yet, if only diarrhœa were added to it, it would serve very fairly to characterize the onset of cholera. But if the symptoms of sea-sickness had been more fully described, the uncommon, as well as the common being included, the likeness would have been still more obvious. The surface of the body becomes pale and cold; there is generally profuse cold sweat, and not unfrequently diarrhœa, sometimes cramps, sometimes, though more rarely, muscular rigidity, and occasionally convulsions. Diarrhœa and cramps are, however, much more frequent accompaniments than seems to be generally supposed. Persons who speak of having been sea-sick, scarcely ever mention the former, but if a number be questioned on the point, it is almost certain that some of them will confess to having been troubled simultaneously with diarrhœa. When sea-sickness ends in death, as is sometimes the case, it generally does so through extreme exhaustion, consequent on the prolonged vomiting, and general impairment of nutrition. Every feature of the disease is also a feature of cholera, and it differs from cholera chiefly in the facts, that diarrhœa is a subordinate instead of a prominent symptom; that all the symptoms, except vomiting, are less severe than are those of cholera; that it is rarely fatal; and that whereas it is always produced by motion, heat, and many other influences, as well as motion, originate cholera.

Now, I have proved by reasoning, and numerous experiments, that the proximate cause of sea-sickness is identical with that which I here affirm to be the proximate cause of cholera. If the application of ice along the spine arrests sea-sickness, it not only proves the correctness of my theory concerning the essential nature of the disease, but it affords a very strong presumption, that inasmuch as the symptoms of cholera are like in nature to those of sea-sickness, and differ from them only by being more excessive, their proximate cause is the same, and that it may be also removed by a method of treatment based on principles identical with those which have dictated a method of preventing or curing sea-sickness. Though the announcement of my discovery, that ice properly applied along the spine will cure sea-sickness, was made two years ago, few persons are, as yet, acquainted with it. It has, however, been amply verified, and as its bearings, by way of confirmation, of both my theory and practice in respect to cholera are of the utmost importance, I commend my readers desirous of studying the subject, thoroughly to acquaint themselves with my exposition of the pathology of sea-sickness, and with the cases illustrative of my treatment of it, contained in my pamphlet on the subject. I have received numerous assurances since this pamphlet was published,

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\* "Treatise on the Practice of Medicine." By G. B. Wood. Vol. i. p. 562.

from persons who have availed themselves of my discovery, that they found it an effectual remedy; and will close this Chapter with the following reports, which do not appear in my pamphlet:—

Captain White, commander of one of the Newhaven and Dieppe boats, in which some of my experiments were tried, says, in a letter addressed to me November 22, 1864,—“I have given your remedy for sea-sickness great attention; and am of opinion that, in ordinary weather, it is a success. I had some difficulty in persuading passengers to try it, but those who did were benefited.”

In the *Lancet* of December 3rd, 1864, is a letter headed “Dr. Chapman’s Remedy for Sea-sickness,” and signed, “S. M. Bradley, Surgeon, Cunard Service.” He says:—

“I have tried this [remedy] *in severe cases where other remedies have failed* (chloroform, iced champagne, effervescing draughts, fresh air, &c. &c.), and have very generally found it do great good. I have applied it to young children, delicate women, and old people. In no case does it do harm; but in the great majority of instances it soothes the nervous irritability which so commonly accompanies sea-sickness, induces sleep, and so enables the stomach to receive light food, and consequently relieves exhaustion. . . . I order it to be kept on a couple of hours; though, as the patient sleeps, as is often the case, I never remove it until after waking.”

The following is an extract from a letter addressed to me, June 3rd, 1865, by Dr. Hayle, of Rochdale.

“I recommended a patient about to cross the Atlantic to try one of your ice-bags for sea-sickness. The result was most satisfactory. He was never sick when wearing the ice-bag. Once he went without it, and then, and then only, was he sick.”

In the latter part of 1865, Mrs. Charles Darwin wrote to me, that her son had recently experienced the benefit of the spinal ice-bag, while passing from Holyhead to Ireland “on a rough morning.” She said, “He is very subject to sea-sickness, and is convinced that, without the ice, he should, on this occasion, have been very bad. He put on the bag soon after starting, when already disordered, and at once felt relief.”

The following letter extracted from the “Medical and Surgical Reporter,” published at Philadelphia, contains a report of the successful treatment of this disease in a case in which severe spasmodic contractions of the muscles of the extremities, with intense pain in the lower part of the abdomen, imparted to the malady a more than ordinary likeness to cholera.

“CASE OF SEA-SICKNESS, SUCCESSFULLY TREATED BY ICE TO THE SPINE.

“To the Editor of the Medical and Surgical Reporter :

“Successful treatment presupposes and involves of necessity the employment of means adapted to the true pathological requirements of the affection treated. But it does not at all follow that he who

proposes or makes use of this treatment, should himself possess a correct view of these pathological conditions. The two ideas are entirely distinct, and we wrong our profession and our patients if we allow ourselves to confound them. A plan of treatment may be strictly and scientifically correct, and yet the arguments urged in its support may be essentially incorrect. We have no right to reject the one because we cannot accept the other. If Dr. Chapman has proposed to us a means of alleviating, with almost perfect certainty, that most distressing malady, sea-sickness, we are criminally neglectful if we refuse to employ it because we do not believe the theories which he advances. And here let me say, before proceeding to detail my case, that medical criticism on both sides of the Atlantic has done itself little credit by the very flippant and unscientific way in which it has discussed this subject. If Dr. Chapman's 'lucubrations' are half as shallow as most of the objections which have been urged against them, they must be weak indeed. And the charge of 'stealing' his ideas, when he has in every instance, so far as I have been able to see, given ample credit to the distinguished physiologists whose views he has adopted, is manifestly unjust. The case which I am about to relate, is as conclusive as a single case can be, in regard to the great practical value of his discovery.

"Early in the present spring I was consulted by a young married lady as to the best means of preventing sea-sickness during a proposed voyage to Havana. She was a person of excitable, nervous temperament and delicate organization; has suffered for a year past from uterine disease; during the winter had been subject to constant attacks of coryza and bronchial catarrh, and was then in the third month of pregnancy. I procured an eighteen-inch ice-bag, and gave her husband full instructions as to its use. It was first to be applied from the nape of the neck directly down the spine, as far as it would reach, all three compartments being filled. I avoided carrying it down to the lumbar region from the fear of producing uterine congestion. If severe headache, with flushed face and throbbing of the temporal arteries came on, the upper section was to be emptied of its ice—the others remaining full: and if pain in the chest supervened, the middle, or even the middle and lower ones were to be emptied, the upper one being full.

"The outward voyage was extremely calm, and our patient suffered only from slight nausea, which, however, was always relieved by the ice-bag. Emboldened by her seamanship and experience, and greatly benefited by her open-air life in Cuba, she determined on her return to make no use of the ice, but by remaining constantly in the fresh air on deck, to bid defiance to the enemy. Unfortunately for her resolution, but fortunately for the reputation of the treatment, the homeward trip was very different from the outward one.

"No sooner did the steamer reach the mouth of the harbour than she struck a heavy cross sea, and from that time until they came into port, four days and a half, they were in very rough water.

"Mrs. — took occasion, as they steamed down the quiet bay, to fortify herself with a good dinner, and on the strength of it went up on deck just as the vessel began to feel the waves.

"She soon became nauseated, and in half an hour was obliged to walk to the side of the vessel and relinquish the meal upon which so much dependence had been placed. This she was able to do unaided. In about fifteen minutes, however, being again called in the same direction, she could not walk without assistance. Violent and distressing retching now set in, with scarce a moment's intermission. She rapidly became prostrate, the blood leaving the head and extremities, which were very pallid and cold, and what was still more alarming, severe spasmodic contractions of the muscles of the extremities, with intense pain in the lower part of the abdomen, set in. I had warned her husband of the danger of abortion from the violent action of the abdominal muscles, and the ordinary remedies for sea-sickness having been exhausted in vain, he now became alarmed and resolved to apply the ice without further delay.

"Obtaining assistance, he carried his wife—more dead than alive, and conscious of but one desire, as far as her anæmic brain was capable of consciousness, namely, that of being thrown overboard—down to the state-room, and had the ice-bag filled in all its compartments.

"The effects of its application were little short of miraculous. In three minutes the retching ceased and the spasms were calmed. In a quarter of an hour she had fallen into a quiet sleep; and in half an hour her hands and feet were of natural warmth, and her face had regained its wonted colour. In two hours she awoke, greatly refreshed, and ate two slices of toast with a cup of tea, and from that time did not miss a single meal. At eleven o'clock that night, slight symptoms of disturbance returning, the ice was at once re-applied, with most satisfactory results. She slept peacefully all night, although the sea was very rough, finding no inconvenience from the cold poultice, *except when it happened to be pushed off the spine.*

"It was applied again before breakfast the following morning, and after this, about five minutes before each meal, being allowed to remain on until the ice was melted, usually about two hours. On the third day she began to experience some pain in the chest, and her husband removed the ice from the middle partition, with the result of its disappearance. A few times it was necessary to apply it between meals, or on going to bed, but generally the three applications daily proved sufficient. I have seen as yet no history of a case in which this method of treatment has been tried on a long voyage, as across the Atlantic. Dr. Chapman's cases were only during the few hours required to cross the Channel. Mine, I think, covers a greater length of time than any yet published; and I can see no reason why the process may not be successfully extended over twelve days as well as four, with proper care and management.

"BENJAMIN LEE, M.D.,

"109, South Broad-street, Philadelphia.

"May 25th, 1866."

## CHAPTER IV.

### CAUSES OF CHOLERA.

1. *Classification of the Causes of Cholera.*—More extensive and precise knowledge than we yet possess is needed before the etiology of cholera can be completely and conclusively discussed; but the facts bearing on the subject and already known, are so abundant, and extend over so wide an area, that a fairly comprehensive statement of them, as the data from which the causes of cholera may be induced, would occupy an amount of space and time far beyond what I can now devote to it. I shall, however, endeavour to sketch the outlines of my views on the subject, supporting them by such facts as are immediately available, in the hope that I may have an opportunity of treating it more fully and satisfactorily on a future occasion.

The causes of cholera are numerous, and their operation is often simultaneous and complex. I believe, however, they may be so classed as to prove, so far as evidence of identity of causes will avail as proof, that summer diarrhoea and cholera are one and the same disease; for it appears indubitable that they both arise out of the same conditions, and are originated by the same influences. The phenomena of cholera are ascribable to remote or predisposing, proximate, and exciting causes. The term, *proximate cause*, is generally understood to denote that morbid condition of any given structure or structures, the presence of which entails the symptoms characteristic of the disease in question as an inevitable consequence. This immediate cause is therefore justly regarded as the essence of the disease; and hence the determination and description of its seat and character constitute pathology. The pathology of cholera having been already discussed, I have only to advert here to its *remote* or *predisposing* and *exciting* causes. These terms are, in my opinion, objectionable; for, in respect to the causes of cholera, at least, they are often interchangeable: thus, assuming the pathology of cholera explained in the preceding chapter, atmospheric heat may induce undue vascularity of the nervous centres, and so predispose them to excessive functional activity; if now the person in whom this change has occurred drinks freely of alcoholic liquors, or water containing a considerable proportion of organic matter, he may directly, through the stimulating influence of the alcohol on the nervous centres, or indirectly through irritating the nerves of the alimentary



tary mucous membrane by the bad water, so excite those centres as to produce cholera; and conversely, alcohol or bad water acting as stated on nervous centres not already predisposed to cholera, may render them hyperæmic, thus inducing in them a predisposition to the disease, and when this condition has been established in this manner, the supervention of great atmospheric heat may excite in them that excessive activity resulting in cholera. Hence it appears, that what is a predisposing cause in one person may be an exciting cause in another, and *vice versâ*. I shall, therefore, not attempt to discriminate between the so-called predisposing and the exciting causes, but shall endeavour so to arrange the several kinds of causes, as to facilitate, as much as possible, an understanding and appreciation of their modes of action. In Part I. of this book different kinds of diarrhœa are arranged according to my conception of their different causes. The various causes of cholera are, I believe, essentially the same as those productive of diarrhœa, but, whereas one only of the causes enumerated in the first part of this book, is very often productive of diarrhœa, the sources of cholera are more frequently complex, and thus effect, by their co-operation, results which, acting singly, they would be unable to produce.

It seems to me it will conduce to the acquisition of the most correct conceptions concerning the etiology of cholera, to range its causes primarily into three classes, as follows:—

*First. EPIDEMIC CAUSES, viz., Solar Heat; Wide Ranges of Temperature; Atmospheric Electricity; Deficiency of Ozone; Malaria.*

*Second. TELLURIC CAUSE, viz., Lowness of Site.*

*Third. ENDEMIC CAUSES, viz., Prolonged Marches, Pilgrimages and Ordinary Travelling on Foot; Noxious Effluvia; Impure Water; Bad Food and Eating to Excess; Dental Irritation; Purgative Medicines; Opium; Alcohol; Habitual Excitants of the Nervous System; Mental Emotions; Influences chiefly operative during the Night; the Tendency of Cholera to recur in the same Individual, and in the same Nation.*

2. *Solar Heat.*—The enormous influence of solar heat in the production of cholera is generally recognised. In those years in which the disease becomes neither epidemic nor frequent, the action of heat in producing what is called summer diarrhœa, which is becoming more and more recognised as the first stage of cholera, is strikingly apparent; and in those same years the sporadic cases of cholera which occur in temperate climates, generally do so in the months of June, July, August, September, and October. On the other hand, in hot climates, bowel complaints are much more prevalent throughout the year: in India, especially in Bengal, they are always numerous, and there is no part of the year in which fatal cases of cholera do not occur. The effects of heat in developing cholera are strikingly evinced in the following statement extracted from the Registrar-General's Report on Cholera in England 1848-9.

*Deaths in England from Cholera during each month that the epidemic reigned in 1831-2, and 1848-9 respectively.*

Months.	Deaths from Cholera.		Of the total deaths from Cholera, the proportion per cent. each month.	
	1831-32.	1848-49.	1831-32.	1848-49.
1831-1848.				
October . . .		354		·651
November . .	97	376	·314	·691
December . .	282	375	·912	·689
1832-1849.				
January . . .	614	658	1·986	1·210
February . .	708	371	2·289	·682
March . . .	1,519	302	4·912	·555
April . . .	1,401	107	4·530	·197
May . . .	748	327	2·419	·601
June . . .	1,363	2,046	4·408	3·761
July . . .	4,816	7,570	15·574	13·196
August . . .	8,875	15,872	28·699	29·178
September . .	5,479	20,379	17·718	37·463
October . . .	4,080	4,654	13·194	8·555
November . .	802	844	2·593	1·552
December . .	140	163	·453	·300
Total . .	30,924	54,398	100·000	100·000

It is justly observed by the Registrar-General: "As the greatest effect on the air appears some weeks after the sun has exerted its power, so the diseases which excessive heat favours appear some time after the temperature of the air has been highest. Thus the temperature rises above the average at the seventeenth week, and falls below the average at the forty-second week; the mortality from summer diseases rises above the average at the thirtieth, and falls below the average at the fortieth week."

As already stated, 4034 persons died of cholera in London during the eight weeks ending September 1, 1866. In the beginning of July the disease had scarcely appeared in London. During the present epidemic (of 1866) in St. Petersburg upwards of 11,000 persons had been attacked up to the end of August, but of this large number of cases only about 2700 had proved fatal.\* The question,—What is the reason of this remarkably small mortality? —is an extremely important one. When we shall learn the facts in connexion with this epidemic, we might, perhaps, find some other answer to the question than the one I am about to suggest; but

\* *British Medical Journal*, September 8, 1866. I quote from memory, and cannot give the exact numbers.

meanwhile it seems not improbable that the correct answer is as follows:—St. Petersburg being in  $59^{\circ} 56'$ , its mean temperature in summer does not exceed  $61^{\circ} 7$  Fahr., therefore when the thermal or thermo-electric states of the atmosphere there are most conducive to the genesis of cholera, the influences originating the disease are, as a general rule, less intense and potent than they are in more southern capitals.

3. *Wide Ranges of Temperature.*—It appears that while a high temperature is, as a general rule, necessary for the production of cholera, it prevails most extensively when in connexion with the high temperature there is a great range between the degrees of greatest heat and greatest cold within each twenty-four hours. "The reader of the meteorological tables," says Dr. Farr, "will not fail to observe, that in the thirty-sixth week of 1854, when the cholera raged, and the deaths from all causes rose to their maximum (3413), the average daily range of temperature was  $30^{\circ} 9$ , consequently the greatest in the fifty-two weeks; the *highest* temperature of the week was  $81^{\circ} 2$ , the *lowest* was  $48^{\circ} 1$ ; therefore the entire range was  $38^{\circ} 1$ ." The great influence of an extensive range of temperature, coincident with great heat, in producing cholera, is strikingly exemplified in Calcutta. The following instructive tables, having exclusive reference to Calcutta, have been published by Dr. John Macpherson.\*

*Table showing Deaths from Cholera for twenty-six years, and from Small-pox for twenty-nine years, &c.*

	Cholera.	Small-pox.	Rainfall.	Temperature.	Range of Temperature.	Prevailing Winds.
			Inches.			
Jan. .	7,150	1,425	0.21	$63^{\circ} 4$	$17^{\circ} 9$	N., N.E., N.W.
Feb. .	9,346	2,845	0.42	$74^{\circ} 2$	$17^{\circ} 3$	N., N.E., N.W.
March.	14,710	4,934	1.13	$82^{\circ} 9$	$16^{\circ} 3$	W., S.W., S.
April .	19,382	4,249	2.4	$86^{\circ} 6$	$14^{\circ} 7$	S., W., S.W.
May .	13,355	2,261	4.29	89	$13^{\circ} 3$	S., S.W.
June .	6,325	1,054	10.1	$86^{\circ} 2$	9	S., S.W.
July .	3,979	555	13.9	$84^{\circ} 1$	6.4	S., S.E., S.W.
Aug. .	3,440	223	14.4	$82^{\circ} 6$	5.2	S., S.E., S.W.
Sept. .	3,935	188	10.4	$83^{\circ} 8$	6.6	S., S.W., W., N.W.
Oct. .	6,211	147	4.72	$81^{\circ} 1$	8.8	W., E., S., N.W.
Nov. .	8,323	132	0.90	$75^{\circ} 4$	$14^{\circ} 2$	N.N.E., N.W.
Dec. .	8,159	576	0.13	$66^{\circ} 9$	$16^{\circ} 4$	Ditto.

Dryness and clearness of the atmosphere, are conditions generally recognised as favourable for the development of cholera. It seems to me probable, however, that these are merely the usual accompaniments of a great range in the diurnal temperature. When the weather is wet, and therefore cloudy, the days are necessarily cooler,

\* "Cholera in its Home," pp. 4—6.

and the nights warmer than they otherwise would be; for the clouds acting as partial non-conductors, shade the earth's surface during the day, and impede the radiation of heat from it during the night. Therefore, evidence that wet and cloudy weather impedes the development of cholera, while dry and clear weather facilitates it, is, in my opinion, only another mode of stating that a great range of temperature facilitates the development of cholera, while a slight one impedes it. But, be this as it may, the fact is indubitable, that in Bengal especially, the occurrence of the least number of deaths from cholera is during the wet months, although the average temperature reaches 83°. As will be seen, during the months of March, April, and May, when the deaths from cholera reach their maximum, the average temperature is only 3 degrees higher. "If," says Dr. Macpherson, "we take the simplest division of the Bengal year, we find that—

Seven dry months yield ..... 80,405 deaths.  
 Five wet months yield ..... 23,890   ,,   "

He gives a further analysis of these results in the following table:—

		Deaths.	Average Tempera- ture.	Range of Tempe- rature.	Rain-fall.
			Degrees.	Degrees.	Inches.
Hot—March, April, May...		47,929	86·16	14·7	2·68
Wet—July, August, Sept.		11,354	83·5	6·06	12·9
Cold—Nov., Dec., January		23,632	68·63	16·1	·413
Transition—Feb., Jun., Oct.		21,882	80·5	11·7	5·08
Transition months in detail. }	February .....	9,346	74·2	17·3	·42
	June.....	6,325	86·2	9	10·1
	October .....	6,211	81·1	8·8	4·72

Three hot and dry months have ..... 47,427 deaths.  
 Three cold and dry months have ..... 23,632   ,,  
 Three hot and moist months have ..... 11,354   ,,  
 While the three transition months have 21,882   ,,

Thus it appears, as observed by Dr. Macpherson, that "dry air, with high temperature, and wide range of the thermometer, is most favourable to the development of cholera; while moist air, with high temperature, and small range, is most unfavourable to it." In 1853-4, as remarked by Dr. Goodeve, "the atmosphere in England was drier than usual, for every month except May and December, the rain-fall being 18·68 inches, or 5·93 less than the average." My opinion, that the mere dryness of the air stands in no immediate causal relation to cholera, and is only remarkable in this connexion,

because productive of, or associated with, those great alternations of temperature, which are peculiarly favourable to outbreaks of cholera, is confirmed by the statement of Dr. Goodeve, that rain and moisture do not prevent the spread of cholera in the North-West Provinces of India, in Bombay, or in Madras. He says, "The North-West Provinces have, at times, suffered severely in the hot and dry months, but the majority of the epidemics have been in the wet months, as shown by the Report of the Commission of 1861." Moreover, during the memorable outbreak of cholera, the dew point, as stated by Mr. Thom in his report, quoted by Dr. Goodeve, "was very high, 83°; with thermometer at 90° in the shade, and there was induced a sense of languor and oppression, a stifled feeling about the respiration, and inability to undergo the least fatigue." Perhaps it will be found, that a very high temperature, with a slight range, or a lower temperature, still high, however, with a great range, are each equally favourable to the production of cholera.

The influence of wide variations of temperature in producing diarrhœa, great activity of the liver, and finally causing such contraction of the hepatic vessels as to arrest the functions of that organ simultaneously with a general contraction of the arterial vessels, is well illustrated by Sir Ranald Martin, in the following paragraph: "Those who have visited Simla, and some of the stations near it on the hill ranges of the Himalayas, have very generally observed a change to a pale, colourless state of the intestinal secretions, soon after their ascent into those regions, resulting, it is presumed, from the comparative cold and damp of the mountain air. Diarrhœa is, in fact, a frequent result of this change of climate, so much so, indeed, as to have received from the British residents there, the name of 'the hill-trot.' Exposure upon active field service, or during the night and the early morning marches in India, especially during the cold season, often produces the same result. Both the conditions here mentioned, the pale secretion and the diarrhœa, would appear to result from hepatic congestion, the consequence of the sudden application of cold and damp to the surface of the body, previously relaxed by the heat of the plains. The result is, a diminished or suspended secretion of bile, and a consequent disorder in the entire process of digestion, the matters voided from the bowels being colourless, acrid, and irritating. Officers of the Bengal army, who marched from Sindh, where the thermometer rose to 135°, to Cabul, where it fell below 75°, have stated to me, that soon after exposure to this relative cold, they all became more or less jaundiced; some were seized with diarrhœa, but all were jaundiced. By some very observant patients, I have been told, on the other hand, that in their instances, diarrhœa commenced in India, by enormous and long-continued 'discharges of pure bile,' which would appear to have exhausted the functions of the liver; then came the 'white flux,' with its complete suspension of the nutritive processes; the anæmia of all the organs and tissues of the body; the anæmic ulcerations (chronic aphthæ) of the mucous digestive surface;

and all the attendant difficulties and dangers, so as to call into trying exercise the toilsome and difficult requirements of the physician.”\*

It being apparently indisputable that solar heat favours the development of cholera, and that the influence of such heat is very greatly augmented in proportion as it is accompanied by a wide diurnal range of the thermometer, the question arises,—what is the *modus agendi* of heat alone, and of heat associated with great variations of its intensity in inducing cholera? It is an established fact, that during epidemics of cholera the greatest mortality occurs among children under one year old. It is equally established that when, what I think it correct to call, the choleraic influence is epidemic, but not sufficiently potent to develop its most characteristic features, its destructive energy under the form, in England, of summer diarrhœa, is almost exclusively expended on children. Why is this? The true answer seems to me as follows: During childhood, the vital processes are at their maximum degree of activity: the circulation is extremely rapid, as every part of the body is making copious draughts on the blood, in order to effect, not only its sustenance, but its growth. The nervous system,† and especially the brain, is increasing with astonishing rapidity, and the arteries throughout the body being in the highest degree vital, are dilatable and contractible to an extent and with a facility far greater than during the subsequent periods of life. Moreover, the process of teething causes a multitude of irritating impressions to be transmitted to the medulla oblongata, and thence to other parts of the nervous system, resulting in a great increase of the already hyperæmic condition of that system. Hence it is that fluxes and refluxes of blood to an abnormal extent in the nervous system of children, and the consequent phenomena constituting most of the diseases of that system, are producible in them by causes which, in later years, are either seemingly inoperative or incapable, in the majority of cases, of producing serious results. Hence it is, that of the 180,902 persons destroyed in England by convulsive diseases during the seven years 1848-54, 164,876 were under five years of age; and hence it is, as I maintain, that the increase of heat during English summers, chiefly operates, so far as the disease in question is concerned, in intensifying the already extremely vascular condition, and, consequently, the functional activity of the nervous system in children, and thus of producing the so-called summer diarrhœa, or *cholera-infantum*, which is often as destructive as cholera itself.

Now the influence of a great increase of atmospheric heat operates to increase the vital activity of all parts of the body, and on the principle expressed in the words—“to him that hath shall be given,”

\* “Influence of Tropical Climates in Producing the Acute Endemic Diseases of Europeans,” pp. 675, 6.

† See remarks on the Spinal Cord in Children in the Section on Opium in Chapter V.

the nervous system, already the most vascular, becomes disproportionately still more so, and, therefore, more active. In this way, the susceptibility of the adult organism during cholera times is made to approach, in a certain degree, to that of the child in ordinary times; while the susceptibility of the latter acquires, through the agency of unusually great atmospheric heat, its utmost possible intensity. When excessive hyperæmia of the nervous centres has been thus established, their functional activity is at its maximum, and may spontaneously originate cholera; and when they do not, as they are rendered excitable in the most extreme degree, causes which in ordinary times would produce but little, if any, obvious effect, are now adequate to induce that morbid exaltation productive of the disease in all its grades. Hence it is, that in hot climates, diarrhœa is always prevalent among adults, as well as children; and that in India, where the most favourable conditions for the development of cholera obtain, though it retires as an epidemic, it is never wholly absent.

In section 18 of this chapter, I shall explain what appears to me to be the reasons why the attacks of cholera are most frequent during the early morning; I may say here, however, that I regard as one of those reasons, the influence on the body of a fall of temperature during the night. There can be no doubt that a rapid decline of heat in the surface of the body, associated with the breathing of air the temperature of which is more or less suddenly lowered, is a condition peculiarly favourable to the advent of cholera. Indeed, there are reasons, not generally known perhaps, even to the profession, which make this condition, what is usually called a predisposing cause, of a more powerful kind than is likely to be at first sight imaginable: it was first proved, I believe, by Dr. Brown-Séquard, with the assistance of Dr. Tolozan, that if one hand be put in cold water, the temperature of the other hand will fall to an appreciable degree; it has been known for ages, not only by physicians, but by a large majority of the women of each generation, that if, during the period of menstruation, the feet are allowed to become extremely cold, the flow will, in many cases, be arrested; and, conversely, when it has been arrested, either in this way or in any other, implying only a temporary functional disorder, the application of heat to the lower extremities will re-establish the flow. As I have stated elsewhere,\* "one of my patients, a very intelligent and nervously sensitive lady, assures me, that when one of her legs is rubbed in order to make it warm, the other becomes so spontaneously." And, I may add, that since this statement was published, I have met with many similar facts. Now each of these instances is an illustration of the law of reflex action: when cold is applied to one hand, the impression made on the nerves of that hand is conveyed to the nervous centres, and produces in them a condition causing them to

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\* "Functional Diseases of Women," p. 15, where this subject is discussed.



reflect the impression received as an impulse through the vaso-motor nerves of the arteries of the opposite limb, and thus causing them to contract, lessens the supply of blood to it, and therefore its temperature; in like manner, when menstruation is stopped by putting the feet in cold water (I know a lady who performed this experiment), or by letting them get cold, the impressions made upon them by the cold is reflected through the vasic nerve centres upon the uterine vessels, which therefore contract, and so arrest the flow. Conversely, the application of heat to the limbs in cases when menstruation is arrested, causes dilatation of the vessels of the limbs, the physiological change thus affected, produces, through nervous agency, a change in the nervous centres related to the limbs, and this change results in a diminution of the motor energy of the vasic nerves distributed to the uterine vessels, which, therefore, again relax and permit menstruation to recur. When friction applied to one limb causes the other to become warm, it does so on the principle just explained; for friction is only another mode of applying heat. Now, these various illustrations of reflex action through nervous arcs related to parts of the vascular system, afford convincing proofs of the proposition, that when cold is applied to the surface of the body generally, it produces impressions upon the centripetal vasic nerves, which, through the agency of their centres, are transmuted into impulses, and as such are radiated over the arterial system throughout the body. The consequence is, that not only the superficial, but the deep-seated arterial ramifications contract under the influence of cold, and thus the energy of the general circulation, and therefore the vitality of the whole organism, is lowered.

Assuming the truth of the proposition just expressed, it at once reveals to us how vast and indeed inevitable is the influence of wide ranges of temperature in causing grave disturbances of the sympathetic nervous centres, and how, when the atmospheric heat is great, sudden or rapid diminution of that heat must produce those special impressions on the nervous centres, peculiarly predisposing them to cause such energetic contractions of the arteries generally, as are likely to herald cholera, if not actually to usher it in. Except during the presence of special epidemics, the bowel affections induced by the heat of summer in Europe, and especially in England, attain only that degree of severity usually characterized as summer diarrhœa; but in the Atlantic cities of the United States, where the range of temperature is extremely great, the disease is commonly of a type so much more severe than its analogue in England, that it is no longer called summer diarrhœa, but *Cholera Infantum*, and is thus described by the eminent American physician, Dr. Wood.

"This affection, though not unknown in Europe, is comparatively so rare as to have escaped the special notice of most of the writers of that continent, and when it occurs, is merely ranked among the cases of gastro-enteric irritation or inflammation to which infancy is

subject. Its frequency and great fatality in this country make it an object of strong interest; and, both among the profession and the people at large, it is universally considered as meriting a distinct designation.

"The complaint usually affects children between the ages of three months and two years, though instances sometimes occur before the first and after the last of these two periods. It is exclusively a disease of the warm season, commencing with the first heats of summer, and ceasing upon the occurrence of cool weather in autumn. It is confined, moreover, almost entirely to cities, and prevails most in those of largest size and most densely peopled.

"*Symptoms.*—The attack of cholera is often preceded for a longer or shorter time by diarrhœa; but sometimes the vomiting and purging commence simultaneously. In fatal cases, of short duration, the vomiting usually continues to the end; but when the disease terminates favourably, or is much protracted, it very often subsides considerably, or ceases altogether, leaving only the diarrhœa behind. Occasionally the disease is exceedingly violent and rapid; the vomiting and purging are almost incessant; the stomach rejects everything swallowed, even cold water; the intervals are marked by great languor and distress, with more or less spasmodic pain of the stomach and bowels; and if relief is not afforded, prostration comes on, with a cool and clammy skin, pallid and shrunken features, half closed eyes, insensibility, amounting at length to coma, and death in three or four days, or sometimes even within twenty-four hours."\*

The pathology of sun-stroke, or heat-apoplexy, is still very obscure, and there are many facts connected with it which point to the conclusion, that, under the common term sun-stroke, at least two quite distinct morbid conditions are comprised;† and accordingly, Dr. Dowler, of New Orleans, distinguishes two different states as *solar asphyxia*, and *solar syncope* respectively. It may be, indeed it is not improbable, that the various states observed and designated sun-stroke, are only phases and variations of one and the same disease. The more the phenomena of sun-stroke are studied, the more, I believe, the conviction will deepen, that the spinal cord and sympathetic ganglia, as well as the brain, are profoundly affected,‡ and that there is a great affinity between it and epilepsy, on the one hand, and cholera on the other. The disease is much less frequently the result of direct solar exposure

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\* "A Treatise on the Practice of Medicine." By George B. Wood, M.D. Third edition. Vol. i., p. 699.

† Sir Ranald Martin has some judicious remarks interspersed with much interesting information concerning this disease, which is also ably and fully discussed in Dr. Aitken's "Science and Practice of Medicine."

‡ A characteristic symptom of the onset of *insolatio*, is the passage of an abundance of pale urine, a symptom of the onset of cholera also, and denoting, as I have shown, hyperæmia of the spinal cord.

than is commonly supposed; it often supervenes, as cholera does, during sleep under a high temperature; many of those who become its victims while awake, are seized early in the morning before sunrise;\* like cholera, it often attacks soldiers on the march, and gains access most readily to those who indulge in excesses of eating and drinking; and, like both cholera and epilepsy, its invasion is most effectually resisted by those whose minds are active and occupied. Moreover, the likeness between the two diseases is observable even after death; in cases of sun-stroke, "an occasional burning heat of the epigastrium complained of during life, and an unusual pungent heat of the surface of the body after death, have been noticed," says Sir Ranald Martin, "by various writers; and I had occasion to remark both at the General Hospital of Calcutta, in the instances of European recruits landed from England in the month of May, and who had free access to the vile rum and arrack of the bazaars, and were seized with what was then termed the sun-fever. These reckless young soldiers exhibited in death the true *calor mordicans* of the older writers." Now, the fact admits of no dispute that this disease, as its name implies, is originated either by heat, or when the patient is in a high temperature. But more than this, it is not only a high temperature, it is the coincidence of great heat, with great and rapid variations in its intensity,† that constitutes the condition peculiarly favourable for the advent of sun-stroke. This malady is unhesitatingly recognised as being exclusively a disease of the nervous system; I have given abundant evidence that cholera is so, and the conclusion seems inevitable, that, inasmuch as they are both produced under like circumstances, and by the same causes, they are both produced in the same way, and that the operation of these causes is no less directly on the nervous system in cases of cholera than it is in cases of *insolatio*, or *heat-apoplexy*. Dr. Rennie‡ observes,—“Having, during three consecutive summers in China, on one occasion in Japan, and several times in India seen cholera and heat-apoplexy occur coincidentally, I have formed the opinion that under certain circumstances, the two diseases occur from analogous causes. In making this statement, what I mean to convey is, that certain abnormal

\* Many are, however, attacked after midday: the cases reported by Mr. Longmore and Mr. Cotton occurred in the afternoon or evening. Aitken's "Science and Practice of Medicine," vol. ii. p. 389.

† Sir Ranald Martin observes,—“In countries near the equator, where the heat throughout the year, though high, is uniform, sun-stroke is said to be of but rare occurrence; a certain amount of variation in the temperature of the seasons would therefore seem to dispose to this disease.” But this is not always the case; “the characteristic feature with regard to temperature, when Mr. Longmore's cases were observed, was the little variation of it night or day.” Aitken's "Science and Practice of Medicine," vol. ii. p. 384.

‡ In his work entitled, "Bhotan, and the Story of the Dooar War: including Sketches of a Three Months' Residence at the Himalayas, and Narrative of a Visit to Bhotan in 1865."

conditions of the atmosphere during warm weather, will, in some persons, develop symptoms the same as those to which the term heat-apoplexy is commonly applied; while in others it will produce those to which it is customary to give the name of cholera—both of them generally equally fatal in result—and their causes, I believe, the operation on the nervous system of the electro-chemical agency originating in the atmosphere.” Dr. Rennie, very logically, proposes to protect the spines, as well as the heads of soldiers in India, “because there are many who feel the effect of the Indian sun as much on their backs as on their heads.”

It seems to me probable that in those cases in which cholera has prevailed as an epidemic during winter—as for example in St. Petersburg\*—a powerful co-operative cause of such outbreaks consisted in the considerable and rapid range of temperature which may have obtained. Moreover, the high temperature at which the inside of Russian houses is kept during the winter, involves the necessity of the exposure of the inhabitants to widely different degrees of heat, inasmuch as the external air is extremely cold, and at the same time renders adequate ventilation extremely difficult, if not impossible. Before an accurate appreciation can be arrived at of the bearing of such exceptional phenomena as the St. Petersburg epidemic on the general proposition, that great heat and wide as well as rapid variations in the degrees of it are the most favourable conditions for the development of cholera, and are often its chief cause, we must become acquainted with the manifold facts of the epidemics in question, and of all the influences which were capable of co-operating to produce the disease; for, as it is the object of this chapter to show, its causes are extremely diverse and numerous; and I venture to express the belief that, when their modus operandi, or in other words, the true pathology of cholera is understood, investigators will, for the first time, obtain a guide likely to lead them to the discovery of the special cause or causes operative in each case. As will be seen in the next section, the development of epidemic cholera is sometimes, at least, clearly coincident with great electric disturbances, and that during one of the epidemics at St. Petersburg a very remarkable disturbance of this kind occurred.

4. *Atmospheric Electricity.*—I am unable to add anything to what has already been published concerning the electric state of the atmosphere during the presence of cholera epidemics. It is indubitable that the electric state of the air is widely different when its temperature is high, from that which obtains when it is low; the frequent electric disturbances manifested in thunderstorms

\* “The cholera rose to a high degree of intensity in Paris in 1832, and in Stockholm in 1834, in months when the average temperature was below 55° Fahr. And the epidemic reached its height in Glasgow and Paisley, and other towns in Scotland, in the middle of the winter of 1848-49, when the temperature must have been lower than it was at the same time in London.”—*Report on the Cause and Mode of Diffusion of Epidemic Cholera*, by William Baly, M.D., F.R.S., p. 34.

which occur during hot weather are sufficiently impressive proofs of this fact. If the proximate cause of cholera consists in hyperæmia or excessive exaltation of the functional activity of the nervous system, the operations of which are by the agency either of electricity, or of an element so intimately allied to it in nature that no characteristic distinctions between it and electricity are recognisable, it is difficult, if not impossible, to resist the conviction, that modifications in the state of atmospheric electricity caused by a great increase of solar heat, or by any other influence, must affect, more or less appreciably, the nervous system of man, when he is within the range of the influences due to such modifications. Indeed this conviction is confirmed by experience: though few robust men are either painfully affected, or experience any physiological results in their own persons, when within the range of obvious electrical disturbances in the atmosphere, there are many persons, especially delicate, so called "nervous" women, who suffer considerably, not only during a thunderstorm, but actually from electrical disturbances, the centres of which are so far off that the disturbances themselves are not even manifest at all to ordinary persons resident in the same place. It is a fact of common observation, that women suffering from functional disorders of the nervous system will, on certain days, feel peculiarly oppressed, and will complain that "there is thunder in the air," when no one else has any appreciation of the fact; and that in such cases, thunderstorms are found to have been actually occurring at the time, at no great distance from the persons in question, though too far off to permit of the thunder being heard, or the lightning seen.

Referring to puerperal convulsions, Dr. Churchill says:—"Atmospheric influence, according to M. Dugès, appears to have some peculiar effect in producing the disease, so that it assumes the character of an epidemic. This is confirmed by the observation of Dr. Ramsbotham, who observes:—'I have repeatedly remarked among the numerous patients of the Royal Maternity Charity, as well as among others to whom I have been accidentally called, that several cases have occurred soon after each other. Whether this fact ought to be attributed to mere chance, or to the agency of some general principle upon the female system, I must leave to others to determine in future; but I am inclined to suspect that it may be ascribed to the latter principle. And here I may be allowed to observe, that I have witnessed the occurrence of several cases during warm weather, at a time when the clouds have been charged with electric fluid; when atmospheric appearances have threatened a thunderstorm, and when perhaps they have ended in one.' And most practitioners will probably have had occasion to remark the occurrence of several cases about the same time, as if they depended upon some general cause."\*

\* "On the Theory and Practice of Midwifery." By Fleetwood Churchill, M.D., p. 484.

"In Paris, after the last revolution, and when the hospitals were crowded with the wounded, a very severe thunderstorm was experienced. The mortality was found to be greater in all the hospitals on the night of the storm, than on any subsequent or previous occasion."\*

Sir Ranald Martin observes,—“When the strong south-west monsoon ceases, and the sky becomes obscured by a film of dark, negatively electrified clouds, and the atmosphere hangs like a weight on the mind and body of the soldier, then it is, as remarked by good observers, that epileptic seizures have always been most prevalent in India.”† And further, he says,—“Thunderstorms have often been observed to arrest the progress and recurrence of heat-apoplexy in barracks and hospitals in which the disease had prevailed for days, and this even where the atmospheric temperature had not been reduced by the storms.”‡ “Dr. Barclay notices that cases of *insolatio* occur with increased frequency immediately before a thunderstorm, and that they cease as soon as the electrical discharge has taken place.”§ These authentic facts prove indisputably that disturbances of the atmospheric electricity exert an extremely powerful influence on the nervous system.

An invaluable collection of accurately observed effects of atmospheric electricity on the human body, has been made by the late Dr. Sestier, in his comprehensive, elaborate, and thoroughly scientific work recently published.|| The large number of patients “struck by lightning,” whose histories he has collected, exhibited the effects of atmospheric electricity in every grade of severity; and a considerable proportion of them, after suffering for various lengths of time, occasionally during long periods, completely recovered. Now an examination of the phenomena induced in these cases, in which the electric fluid operated suddenly, and with violence, sometimes extreme, sometimes scarcely more than to produce a painful shock, may enable us to answer the question,—is it probable, *à priori*, that modifications, or disturbances of the ordinary electric state of the atmosphere, can so affect the nervous system of man, as to induce cholera?

It appears, according to Dr. Sestier, although the analogy is not pointed out by him, that nearly every symptom of cholera is observable in one or another of the patients struck with lightning; and is, indeed, of very frequent occurrence. Faintness, syncope, singing in the ears, deafness, cramps, convulsions—both tonic and

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\* “Cholera: its Causes, Prevention, Non-contagiousness, and Treatment.” By M. J. Maccormack, A.B., M.B., 1853, p. 30.

† “Influence of Tropical Climates,” p. 392.

‡ *Ibid.* p. 401.

§ Aitken's “Science and Practice of Medicine,” vol. ii. p. 385.

|| “De la Foudre, de ses formes, et de ses effets sur l'homme, les animaux, les végétaux, et les corps bruts, des moyens de s'en préserver, et des paratonnerres.” Par le Docteur F. Sestier. Rédigé sur les documents laissés par M. Sestier, et complété par le Docteur C. Mehu. 2 vols., 8vo. Paris, 1866.

clonic—are, as might be expected, very often observed. Dyspnœa, præcordial oppression (des anxiétés précordiales, une sensation douloureuse de serrement au cœur), and oppressive pain at the epigastrium, are scarcely less frequent. Impairment or extinction of the voice, said to be due to “le spasm et la contraction tétanique des muscles qui président à la production et à l’articulation des sons,” is a notable symptom, as is also dysphagia “due to the convulsive spasm of the organs of deglutition”—a symptom mentioned by Dr. Macpherson as having been observed in cases of cholera. Vomiting is one of the most usual symptoms, and amongst the matters ejected from the stomach, mucus is mentioned first. Gaseous distension of the abdomen, vermicular movements, and movements as of spasmodic contractions of the intestines, are frequently observed, and, of all the symptoms produced by attacks of atmospheric electricity, diarrhœa, says Dr. Sestier, is the most frequent. He quotes an author of the name of James, who says he has known many persons in whom thunder produced the effect of a purgative, and he mentions that Scaliger, as well as Aristotle, had already pointed out this effect. He also states that Carman, who experimented on birds with electricity, found that one of the first results was expulsion of the contents of the bowels; and that if the shocks were continued, the ejections, which at first were of ordinary consistence, became less so, and finally, completely liquid. The only recorded effects on the liver, appear to have been pain in the hypochondriac region, and in several cases, jaundice. Suppression of urine is a common occurrence, but the most remarkable analogy between the phenomena induced by lightning, and those characteristic of cholera, obtains in the vascular system. The changes relative to the circulation which supervene immediately, or soon after, a stroke by lightning, are described in the following paragraphs, which I translate literally from Dr. Sestier’s work.

“At the moment of the accident, the thunderstruck generally present a condition of collapse, and of algidity (refroidissement), which may continue many hours; sometimes the pulse is feeble, sometimes imperceptible; sometimes soft and easily compressible; sometimes, having these characteristics, it is frequent, but oftener it is *remarkably slow*. Sometimes also it is small, intermittent, and as if convulsive; in two cases cited by Tzschirner and Parkinson, the radial pulse could scarcely be felt, and nevertheless the pulsation of the carotids was very appreciable, slow, regular, and even normal. At the same time, the temperature of the body is below the average, cold, and even icy, especially in the extremities; and the sweat, when it exists, is cold and clammy (*visqueux*).

“At the end of a variable time, often some hours, this state of depression is succeeded by one of reaction, more or less energetic and prolonged: then the pulse is frequent, hard and full; the heat gradually increases, and becomes burning; and sometimes a copious sweat bedews the patient. A circumstance very curious to notice here, is a *fœtid* odour which, in some cases, this sweat



diffuses. Nevertheless, the fever soon subsides, and gives place to sleep, unless it be prolonged and aggravated, or reproduced, by the burns or wounds on the surface of the body, or by the appearance of some 'phlegmasies intenses.'"

These very remarkable facts, seem to me to show, that it is extremely probable, *à priori*, that modifications, or disturbances of the ordinary electric state of the atmosphere, can so affect the nervous system of man, as to induce cholera.

It will be observed, that, in two cases mentioned, the heart was beating healthily, as proved by the pulsations of the carotids, which "was very appreciable, slow, regular, and even normal," and yet the radial pulse could scarcely be felt. This fact is, in my opinion, only explicable by the assumption that the small arteries were spasmodically contracted. The general coldness observable in these cases, is a strong confirmation of this opinion. Indeed, the production of vomiting, purging, cramps, cold sweat, algidity, and reactionary fever by lightning, proves most indubitably that there is scarcely a single feature of cholera which atmospheric electricity does not originate in one or another of those whom it attacks.

"I noticed," says Sir James Murray, M.D.,\* "that during some weeks, when nocturnal lightnings flashed in the horizon, almost all the poor applicants for advice were affected by one uniform train of disorder, such as diarrhoea." Dr. Maccormack says:—"It has been proved that the needle did not obey its usual natural attractions in Russia, during the late awful visitation of cholera; and it was also found that the magnet, which ought to sustain a weight equal to seventy-five pounds to the square inch, gradually lost this power as the disease increased, until at last, when the disease was at its height, it had only the sustaining power of fifteen pounds; as the disease decreased, it regained itself, until it sustained its proper weight; the same circumstance was remarked in Ireland, during the epidemic of 1849, and was published in the medical journals of that period, by (I think) the late Dr. Griffin of Limerick. That cholera has, on more than one occasion, succeeded a severe thunderstorm, is a fact that has been remarked by many authors, and I myself have repeatedly found such to be the case, especially in India, and at those periods of the year when the disease was not epidemic. When the epidemic raged in Ireland in 1832, its outbreak in the town of Sligo, where it spread with unparalleled severity, was preceded by a terrific storm of thunder and lightning; and this occurred also in several other districts throughout the country."†

5. *Deficiency of Ozone*.—The coincidence of a minimum quantity of ozone in the air during the presence of cholera, has been frequently observed. "In 1855, at Strasbourg," says Mr. Alnutt, "the in-

\* Quoted by Dr. Maccormack at p. 32 of his pamphlet, mentioned above at p. 147.

† *Ibid.*, pp. 33-4-5. I find at p. 17 of this pamphlet the following remarkable statement by Dr. J. C. Atkinson, quoted from the *Lancet*:—"I am desirous at the present moment of directing the attention of your numerous scientific readers to a very interesting phenomenon, more or less present in the collapse

vasion of cholera coincided with a period of antozone, and the decline of the epidemic was accompanied by the return of ozone.\* Dr. Herbert Barker, of Bedford, in a letter which appeared in the "Times," in August, 1865, observed,—“It is very remarkable, that during the prevalence of cholera in any district, ozone has been observed to be absent in that district, not the smallest trace has been discoverable by the test papers. On the other hand, it has been noticed, that the epidemic catarrh, characterized by all the ordinary symptoms of a ‘cold in the head,’ has been accompanied with an excess of ozone in the air. On several occasions, when experimenting with large quantities of ozone, by means of Siemens, and Ruhmkorff’s apparatus, I have been suddenly seized with catarrhal symptoms, clearly traceable to its excess. These effects are due alone to an excess of ozone, and are not to be apprehended from the moderate use of the agent.”

In the valuable Lectures on Cholera, by Dr. Bell,† he shows that “its precursor, in a very remarkable manner in Europe and America, was influenza, which pursued, also, very much the same course as the cholera in those continents. This order of appearance of these two great epidemics has occurred once before in India, in the latter part of the last century.” Assuming that the order of succession of epidemics of influenza and cholera accords, even in a considerable proportion of cases, with Dr. Bell’s statement, there would be reason for concluding that the maximum and minimum amounts of ozone in the air during these periods may be regarded as examples of action and reaction in the develop-

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stage of cholera, which seems to have hitherto escaped the observation of medical men—an animal electricity, or phosphorescence of the human body. My attention was attracted to the subject during the former visitation of that fearful disease in the metropolis in 1832. It was, indeed, singular to notice the quantity of electric fluid which continually discharged itself on the approach of any conducting body to the surface of the skin of a patient labouring under the collapse stage, more particularly if the patient had been previously enveloped in blankets. *Streams of Electricity*, many averaging *one inch and half* in length, could be easily educted by the knuckle of the hand when directed to any part of the body, and these appeared in colour, effect, crackling noise, and luminous character, similar to that which we are all accustomed to observe when touching a charged Leyden jar. I may remark the coincidence, that simultaneously with the heat of the body passing off, the electricity was evolved, and I am therefore led to ask the questions, Are not heat, electric and galvanic fluids, one and the same thing? does not the fact of the passing off of both imponderable substances at one and the same time, strengthen this conclusion? Again, are not the whole of what we call *vital* phenomena, produced by certain modifications of the electric, galvanic, and magnetic matter and motions? and do not we find that these *vital* phenomena are continuously affected by the relative state of the surrounding electric medium. To what can we attribute the present fluctuating condition of the barometer, if not to it?”—*Lancet*, November 4th, 1848. I am not aware that any other physician has observed the extraordinary phenomena here reported; and yet, were they really observable, it is difficult to understand how they have escaped attention.

\* Letter to the *Times*, August 24th, 1865.

† “Lectures on the Theory and Practice of Physic.” By Drs. Bell and Stesk. 2 vols. Philadelphia, 1848.

ment of this important element. The general opinion is, that the absence of ozone during cholera epidemics results from its combination with the hypothetical cholera poison. But as the existence of the poison is a mere assumption, of course this alleged cause of the absence of ozone is the same.

Dr. Barker says, that "in the neighbourhood of cesspools all evidence of the presence of ordinary atmospheric ozone is lost, as though it, the ozone, was being expended in overcoming or resolving the emanations arising from the decomposition of the cesspool. Further, it is certain that the absence of ozone in these localities does not depend on the absence of it in the air surrounding, as is proved by the circumstance, that on simply making an ascent the ozone appears. When ozone is abundant in the air, it may be detected on the windward side of a stable, or cowshed, or manure heap, but not on the leeward side. It may be observed abundantly immediately on the windward side of a town, and not a trace of it discovered at the same time on the leeward side." Facts of this kind have given rise to the opinion, that the normal amount of ozone is lessened by its combination with the "cholera miasm." But though this hypothesis may seem to account for the absence of ozone, its superabundance at other times, during epidemics of influenza, for example, justifies the opinion that its excess and deficiency are due to causes, probably of a cosmical nature, with which we are as yet unacquainted.

Catarrhs and influenza are forms of congestion, or sub-acute inflammation: "it is evident," says Dr. Copland, "from an attentive consideration of its phenomena, that the influenza partook largely of the characters of an epidemic fever." Now this condition implies excessive activity of the oxydizing processes throughout the body, and of the circulatory system, and, according to the neuro-physiological doctrines explained in the Introduction to this volume, *anæmia* of the sympathetic nervous centres. These physiological states are precisely what might be predicated as producible by breathing air, the oxydizing power of which is excessive, and such is the character of the air when it contains a maximum amount of ozone.

On the other hand, when the amount of ozone in the air is at a minimum, the oxydizing power of the air is also. The consequence is, that when air of this kind is breathed, the motive power of the circulation,—viz., the attraction between the blood in the pulmonary arteries, and the air in the pulmonary air-cells, and between the blood in the systemic arteries and the tissues about to be transformed—declines; hence, not only is the oxydizing quality of the air lessened, but, as the rate of movement of the blood through both the pulmonary and systemic capillaries becomes slower, a less amount of oxygen is brought in contact with the tissues in a given time; the inevitable diminution of vitality, and therefore of heat, throughout the body, produces, as explained in the section on the *range of temperature*, an irritating effect on the vasic nerve centres,

an effect which they reflect in the form of motor impulses radiated over the vascular system, thus causing an increase of those arterial contractions already initiated by the fall of temperature, which, if not easily appreciable by the thermometer, *must*, nevertheless, result from the impairment of the oxydizing energy of the air respired, and the consequent retardation of the circulation of the blood.

Assuming the facts established, that during the presence of epidemic influenza there is a maximum amount of ozone in the air, and that during the presence of epidemic cholera there is a minimum, it seems impossible to escape from the conclusion, that the operation of the maximum and minimum quantities on the human organism must be of the kind respectively described in the two preceding paragraphs. If so, the absence of ozone from the air must not only operate powerfully as a negative influence productive of cholera, but its mode of producing it is rendered completely intelligible.

6. *Malaria*.—I avail myself of Dr. Macpherson's judicious observations on this subject: "Although we are entirely ignorant of the nature of malarious influences, and cannot reduce them to laws of temperature or of electricity, of moisture or of atmospheric pressure, still their existence is generally assumed, just as that of a cholera poison. That such influences have their share in originating cholera is admitted by all Indian observers; to multiply examples, or to insist at length on this point, appears to me to be quite unnecessary.

"I would merely say that inundations have always been considered the great generators of malaria, when the waters begin to dry up; and that in a Bengalee village, after an inundation there is sure to be a severe epidemic of fever, or of cholera, or of both together, or of first one and then the other. Dr. Wise, writing of Eastern Bengal, says, when fever and dysentery were very common and fatal in the jungle, cholera was committing great ravages along the exposed parts of the retiring river Megna. In such cases there can be no more suspicion that the cholera had been imported than the fever. Inundations in Lower Bengal take place early in September, and they begin to dry up in October and November. This must not be left out of sight, in considering the increase in the amount of cholera among natives at that season." \*

As there seems to be good reason for believing that malaria is one cause of cholera, the questions arise—How does it produce those specific effects constituting the disease? and inasmuch as we know it produces intermittent fever, how is it that sometimes it produces the one disease, and sometimes the other? I shall suggest what may perhaps prove the correct answers to both these questions. There can be no doubt that malarious influences exert their baneful effects by coming in contact with the extensive surface of the pulmonary mucous membrane, just as bad water injures the

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\* "Cholera in its Home," p. 20.

organism by coming in contact with the alimentary mucous membrane. There may, however, be a considerable difference in the mode of action on the two membranes by the organic impurities in water in the one case, and by the aëriform poisonous influence in the other. But though the extreme subtlety of malaria eludes discovery by the most delicate chemical tests, and though it may really operate by gaining access to the blood and exercising a morbid influence upon it, and through it on the nervous centres, it is equally conceivable and, I think, quite as probable, that its immediate effects are wrought by modifying the state of the large expanse of nerve fibres presented by the pulmonary mucous membrane. But by whichever of the two modes mentioned the poisonous influence in question is exerted, the result is the same, viz., disordered action of the nervous centres. This general statement applies equally to the genesis of cholera and of intermittent fever, by malaria; and perhaps in the whole range of human maladies there is no disease which presents more striking evidence than ague does of primary disorder of the nervous centres. In fact, instability or irregularity in respect to the amount of blood passing through the sympathetic nervous centres, is the chief characteristic of this disease. Hence the violent spasmodic contraction of the systemic arteries constituting the rigor or cold stage of the malady, and denoting hyperæmia of the sympathetic ganglia; and, again, the powerful reaction resulting in general arterial dilatation, constituting the hot stage, and denoting anæmia of the same ganglia. Now, the fact that disorder of the nervous system, as just explained, is the proximate cause of ague, is a strong proof that when malaria, instead of producing ague, produces cholera, it also does so by disordering the nervous system. The two diseases, according to the hypothesis here expressed, are intimately allied to each other in nature; and it is not difficult to conceive that when malarious influences co-operate with the other causes of cholera already or about to be described in this chapter, the product of these joint agencies may be cholera; whereas, when malaria acts alone on the human body, it may always generate disease of a character approaching much more nearly to typical intermittent fever than that which cholera presents. These considerations explain how it is "that in a Bengalee village, after an inundation, there is sure to be a severe epidemic of fever, or of cholera, or of both together, or of first one and then the other;" and may supply an answer to Dr. Macpherson's question, "If malaria produces cholera when it is most abundant at the end of the year, why is cholera far most frequent in the hot months, when malaria is considered to be least so?" Excessive heat, wide ranges of temperature, and electrical disturbances, with deficiency of ozone, are abundantly adequate to produce cholera without the aid of malaria; and hence do so when it is least abundant; but inasmuch as in Bengal the heat is always great, it co-operates with malaria at the seasons when it is most abundantly generated, and the product of their combined influence

is cholera, or fever, or both, or first one and then the other, accordingly as the one or the other cause predominates.

In my lecture on Cholera to the South Hants Medico-Chirurgical and Microscopical Society, delivered at Southampton, October 5th, 1865, I said, "Whenever feverish reaction follows the algide symptoms, it may be properly regarded as the second phase of a fever, the cold stage of which is a very prolonged one. The reaction of cholera is not, however, the working of a zymotic poison, but is due exclusively to the fact that the excessive and persistent energy of the nervous centres, becoming at length exhausted, they permit not only of the relaxation, but of the extreme dilatation of the arteries, and the consequent lighting up of those chemical processes in all parts of the body, which, in excess, constitute fever in its most active form." Dr. Macpherson observes, "the resemblance of an attack of cholera to that of an aggravated ague fit has suggested itself to many, and a parallel has often been drawn between them. This view was developed with much ingenuity by Dr. C. W. Bell. But another and more striking analogy is presented in cases of what the French call *fièvre algide*, which are supposed to be dependent on miasma; of this also I have seen some instances. This is the history of *fièvre pernicieuse cholérique* in Algiers, according to Haspel, and such cases are mentioned by Riverius.

"More or less rigors open the scene; next come vomiting and dejections, bilious, yellow, or green in colour, pain in the abdomen. The features of the face become, in a few moments, pinched; the eyes hollow, with cavities around them; the voice extinguished; a bluish colour and intense cold occupy the inferior extremities, and spread over the whole body. The tongue, which is large and moist, gets icy cold, at the same time a burning heat is felt in the interior, with ardent thirst; the pulse is small and thread-like, to be felt only at intervals, and soon ceases to be felt at all. The urinary secretion is suppressed, painful cramps attack the calves, and the dorso-lumbar region, and extort groans and cries. The patients complain of cardialgia, of an inexpressible feeling of suffocation, and constriction of the base of the thorax, which seems to prevent the entry of air into the chest, which causes great anxiety and extreme agitation; constant vomiting and purging come to add to their sufferings. The respiration becomes more embarrassed, gets weak, and is by degrees extinguished. The weakness of the heart gradually increases, and its pulsations become almost imperceptible, a cold perspiration inundates the patient, and he expires asphyxiated, with his consciousness usually remaining complete in the midst of all this perturbation.

"When the case does not terminate fatally, reaction suddenly sets in, the pulse begins to return slowly, the skin begins to soften and regain its natural tint. Slow, regular movements of the heart commence to be heard, and the respiration becomes freer and deeper; the face loses its cadaverous colour, the vomiting has disappeared, and the alvine dejections have ceased entirely. If the

reaction passes certain limits, one often sees typhoid symptoms develop themselves. In a fatal case, the right cavities of the heart were found gorged with coagulated black blood. Here have we not almost a complete picture of ordinary cholera? But there are differences. Cholera is not ushered in by rigors, the fluids ejected are yellow, green, or bilious, in pernicious fever; in cholera, like rice-water; the face, too, is not so much altered in the fever as in cholera; and, above all, the specific treatment by quinine is only applicable to a miasmatic fever.

"I have met with many cases of fever suddenly terminating in hopeless collapse. Martin mentions the prevalence of a choleraic fever in Calcutta during one season. In his recent report on the great epidemic fever which has of late years devastated Lower Bengal, Dr. Elliot says, he was informed that the cases which were most rapidly fatal varied little, in symptoms, from cholera. Unfortunately he did not see any of them. But Surgeon R. H. Perkins tells me, that he has often known fever in the Hidgeelee district commence like cholera, so that the diagnosis might at first be uncertain."\*

It is worthy of observation that persons newly arriving in Bengal are found to be more liable to be attacked with cholera than natives, or persons some time resident in the country. This special liability, observed also, though in a less degree, in Europe, is, according to Dr. Macpherson, a distinctive feature in Bengal. "Now, there is one characteristic of malaria," says Dr. Parkin, who has given especial attention to the subject, "that requires to be mentioned; and this is, that strangers, or those coming from another and more healthy locality, are sooner and more easily affected by the poison than those accustomed to inhale the infected atmosphere."† It seems to me that the peculiar liability of new-comers in Bengal is due to the fact, that malaria, as one of the causes of cholera, plays a more important part there than it does in most other places; for, irrespective of it, other causes of the disease are always more or less efficient in favouring its development in Bengal: and hence, when malaria is superadded, the chances of an outbreak are increased, and increased precisely by that cause most likely to affect new-comers with disproportionate severity. Perhaps more precise observations than have yet been made will ultimately establish the fact, that the liability to cholera of new-comers is greatest in Bengal during that part of the year when malaria is most abundantly generated, and that at other seasons their immunity from the disease approaches that of residents in the country.

7. *Lowness of Site*.—This subject has been fully investigated by Dr. Farr. He observes: "The elevation of the soil in London has a more constant relation with the mortality from cholera than any

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\* "Cholera in its Home," pp. 65, 66, 67, 68.

† "The Causation and Prevention of Disease." By John Parkin, M.D. 1859, p. 94.



other known element. The mortality from cholera is in the inverse ratio of the elevation. The mortality of the nineteen highest districts was at the rate of 33 in 10,000, and of the nineteen lowest districts 100 in 10,000. The elevation in the two groups was as 71 to 10 feet above the high-water mark of the Thames, or as 7 to 1; while the mortality was as 1 to 3, or in inverse ratio.\* The following table exhibits the mortality from cholera in 1848-9 in the metropolitan districts ranged in terraces according to their elevation.

Elevation of districts in feet.	Number of terrace from bottom.	Deaths from Cholera in 10,000 inhabitants.
Feet.		
20—	1	102
20— 40	2	65
40— 60	3	34
60— 80	4	27
80—100	5	22
100—120	6	17
340—360	18	7

"Elevation of the land," says Dr. Farr, "involves several conditions which have an important effect on life and health. As we ascend, the pressure of the atmosphere diminishes, the temperature decreases, the fall of water increases, the vegetation varies, and successive families of plants and animals appear in different zones of elevation. The waters roll along the surface of the rocks, or filter through them and the porous strata of the earth to burst out below—the sources of rivers, or of tributaries which carry disintegrated rocks, with the remains and excretions of vegetable, animals, or men in every stage of decomposition. The deposits in stagnant places, and at the estuaries, show the kind and quantity of mixed matter which the laden rivers carry down and deposit on the low margins of the sea at the tidal confluences of the fresh and salt waters." Dr. Goodeve remarks as a matter of fact, that "The most favoured seats of cholera all over the world are places not high above the sea; along the banks of rivers, and the estuaries of great streams. \* \* \* Cholera is less prevalent in mountain elevations than on low levels. This is the case in the Indian hill stations. They have not, however, been exempt from severe visitations."\*

8. *Prolonged Marches, Pilgrimages, and ordinary Travelling on Foot.*—In Chapter II., Part I., I have shown that motion, as exemplified in the movement of ships, and in certain cases in excessive walking, will produce diarrhoea. That cholera may be produced in the same way, is proved by the following authoritative statement of facts. "It has been proved in India, and recorded by Mr.

\* ART. "Epidemic Cholera." By Edward Goodeve, in "Reynolds's System of Medicine," vol. i. p. 132.

Orton, Drs. Balfour and Lorrimer, that troops, both European and native, suffer more from cholera on the march, or soon after it, than they do at the military stations. Dr. Balfour has proved, that of the native soldiers of the Madras army, 32 died of cholera in cantonment, and 86 when marching, to an average of 10,000 in strength. Dr. Lorrimer's reports show that the troops were more frequently attacked on long than on short marches:—thus the troops, in 219 marches of 20 to 40 days, were attacked 39 times; while, in 14 marches of 100 to 120 days, they were attacked seven times. If we take a hundred marches as the basis, they were attacked 18 times in about 30 days, in the one case; while, in the other case, they were attacked 50 times in about 110 days; that is, at the rate of 14 times in 30 days.\* “The adherence of cholera to troops moving, seems at variance,” says Dr. Goodeve, “with the beneficial effects so often experienced in moving troops out of infected cantonments in cholera epidemics. \* \* \* It has been known that a regiment suffering severely from cholera in camp on the march has lost it on getting into barracks. A remarkable instance of this occurred in the case of H.M.'s 63rd Regiment, which suffered extremely during the greater part of its march from Poonah to Bellary; but entirely lost the disease in two or three days after getting into barracks in the unhealthy station of Bellary. \* \* \* H.M.'s 86th Regiment, which suffered so severely at Kurrachee in 1846, had come off a long and fatiguing march from Upper Scinde.” Dr. Macpherson testifies that “those who are travelling are specially liable to be attacked. \* \* \* It is notorious that among Europeans more cholera occurs in hotels and lodging places than elsewhere. Many of these houses are no doubt defective in sanitary arrangements; but I believe that the fact is attributable more to something in the condition of the travellers than to the state of the houses they resort to. I have seen persons from the Provinces die of cholera in Calcutta in the airiest and loftiest private houses; and this too when no particular fatigue had been undergone. Under this head come travellers dying at staging bungalows; also, at least, as one cause, the readiness with which, in certain districts, both European and native troops get cholera on the line of march. It is well known how pilgrims on their way through Lower Bengal and Orissa strew the road to Juggernaut with their bones. Natives travelling are just as likely to suffer as Europeans.” Referring to the effects of Hindoo pilgrimages, a writer in 1844† says:—“We are well satisfied that for the last eight or ten years the prevalence of cholera in this district [Ahmednuggur] can be traced almost universally to the pilgrimages made to Sunderpoor. Whenever a pilgrimage occurs, there cholera almost

\* “The Influence of Tropical Climates in Producing the Acute Endemic Diseases of Europeans.” By Sir James Ranald Martin, F.R.S., p. 525.

† Quoted by J. N. Ratcliffe, in the “Eighth Report of the Medical Officer of the Privy Council, 1865,” p. 370.

invariably breaks out among the assembled crowds; people become frightened, and return with all speed to their respective villages; and in a few days you will hear of the cholera in this village and in that all over the district. Many pilgrims die on the road as they return to their homes, and when they stop for the night the cholera is sure to commence immediately." That prolonged walking is generally one of the chief causes of cholera among pilgrims I have no doubt; but that it is only one of many causes is well exemplified in the following passages:—"People undergoing every fatigue and toil of long journeys, with privation of food and rest, arrive at their tabernacle half starved or much exhausted, and have a small enclosed room, pointed out by the pundahs, for the accommodation of twenty or thirty persons; as soon as they have their baggage secured they repair to the temple, washing themselves in different stinking ponds, and wearing wet or silk clothes (as an undefiled dress), visit the image, eat the various foods with eagerness, taking no notice whatever of its condition, taste, or quality, under a well-impressed idea that such observation would be an act of blasphemy, drink a jumboo-full of very sour rancid tyre, and feel themselves refreshed and very much satisfied for the first day; but from the second or third day the causes of diarrhoea, or cholera, well known to the medical world—viz., sudden transition from heat to cold, aliment of indigestible character, acrid food or acrid drinks, oily and putrid substances, and want of free ventilation and drainage—soon operate upon the supposed repenters of sin.

"The streets and houses are impregnated with noxious exhalations emanated from the decomposition of the excrementitious and urinous deposits with which the streets, valleys, fields, and plains, are filled during the assemblage of people in great numbers, as well as from the dead bodies thrown out in the fields, and in the towns.

"Cholera having thus originated, great alarm and despair are produced among the pilgrims, and the fright and despondency on one hand, and their longings for home, relatives, or friends, on the other, act conjointly as depressing agents, rendering their system more favourable to the action of the causes of cholera."\*

In Chapter II. of Part I., I have explained the *modus operandi* of the motion of ships in producing diarrhoea; but the production of cholera by long marches, by pilgrimages on foot, and by the ordinary muscular exertion incident to travelling, needs special explanation. And first, I will advert to a fact, seemingly far from the subject, but, in truth, very pertinent to it, as well as illustrative and confirmatory of the explanation I am about to give, viz., that there are many women who, if they walk to any considerable extent while menstruating, will by this means completely arrest the flow. Now, as it seems to me, the only possible interpretation of this fact is as follows: the excito-motor, or all but automatic activity of that

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\* Extracted from a letter of a Native Medical Subordinate on the Madras Establishment, W. Vencataswamy, quoted by J. N. Ratcliffe.

part of the spinal cord especially related to the lower extremities, induces in it a considerable afflux of blood; the proximate sympathetic ganglia become affected in like manner; their action on the blood-vessels becomes therefore more energetic, and those of them which are specially related to the arteries of the womb, stimulating those arteries unduly, cause them to contract so vigorously, as greatly to lessen or all but cut off the blood supply to that organ. Hence, of course, the arrest of menstruation. Now, this special effect of the special activity of the lower segments of the spinal cord, which is a condition precedent of walking, exemplifies how the hyperæmic state induced in the spinal cord, is extended to nervous centres not functionally implicated in effecting locomotion. And it will now be no longer difficult to understand that if the uterine arteries can be made to contract so completely as here described by the moderate amount of walking which women, under the circumstances in question, are likely to indulge in, the prolonged marches of soldiers, and of pilgrims to Hindoo or Mahomedan shrines, are likely to result in such a hyperæmic condition of the numerous sympathetic ganglia in proximity with the lower half of the spinal cord, as will cause them to induce energetic contractions of the arteries which they govern, and which are distributed to the abdominal and pelvic viscera, as well as to the lower extremities. Moreover, inasmuch as the lower segments of the spinal cord themselves are in a state of extreme functional activity, they must exercise a preternaturally powerful stimulant influence over the whole glandular structures of the intestinal mucous membrane. When it is considered that the marches and pilgrimages in question occur in hot climates, where the various causes of cholera already enumerated are more or less active, it will be at once seen, that so far from there being any mystery in the fact that marches and pilgrimages conduce to the development of cholera, the wonder is, that they do not do so more frequently and more abundantly.

9. *Noxious Effluvia* as causes of cholera are mentioned by almost all writers on the subject. That they often prevail without producing cholera is fully admitted; but that they are powerful co-operative agents of its production, no one can doubt who studies their effects on the human organism by the light of the doctrines expounded in this work. Sickness and vomiting are induced in many persons instantaneously after they have swallowed a small quantity of air vitiated by exhalations from excrementitious matter; and in many more the mere inspiration of such effluvia for a few seconds produces a profound depressing effect on the system—often accompanied with considerable gastric disorder. "In spite of exceptions," says Dr. Goodeve, "the places in which the air is most vitiated from privies, cesspools, drains, decaying animal and vegetable refuse, or overcrowding and concentration of human emanations, are those in which cholera has generally been most fatal and most widely spread."



10. *Impure Water*.—That this is a cause of cholera has been proved by such an overwhelming amount of evidence, that no doubt on the subject can still be entertained by any competent judge. Moreover its causal efficacy in cases in which it cannot have been previously contaminated by discharges from the alimentary canal of cholera patients is so completely established, that it is wholly needless to recur to Dr. Snow's far-fetched doctrine, that a cholera poison generated in the intestines cast out with the discharges, and mixing through the medium of drains, or otherwise, with water which is afterwards drunk, reproduces and diffuses the disease. A very remarkable instance of the cholera-generating power of impure water is given by Mr. Simon, and is cited from his report by Dr. Goodeve. "The Lambeth Water Company drew its supply from the Thames at Ditton, above the influence of the London sewage and tidal flux; the Southwark and Vauxhall Company drew its supply from the river near Vauxhall and Chelsea. The water of the Lambeth Company was tolerably pure, that of the Southwark and Vauxhall Company was very impure. The water of both Companies was distributed in the same district, at the same time, and among the same class of people, the pipes of the two Companies being laid pretty evenly in the same areas, in many places running side by side in the same streets, and the houses supplied pretty equally distributed. The deaths in the houses supplied by the Lambeth Company were at the rate of 37 to every 10,000 living; in those supplied by the Southwark and Vauxhall at the rate of 130 to every 10,000 living. The population drinking the foul water appears to have suffered  $3\frac{1}{2}$  times as much as that drinking the purer water. This seems to be an experiment as free as possible from error. The population submitted to the test being 400,000—500,000; the only ascertainable differences of circumstances in the two classes being in the kind of water supplied. The well-known instance of the sickness following the use of the water of a pump in Broad Street, Golden Square, affords strong evidence of the evil influence of water contaminated with cesspool drainage."

"The source of the evil is found in the organic impurities either visible or invisible; and whether we adopt the doctrine of Liebig that the effects are produced by a species of fermentation set up by the introduction of decaying matter into the system, or the opinion that it acts directly as a poison, the fact remains the same. The noxious influence of this matter is increased by its solubility in water. Water at  $32^{\circ}$  dissolves scarcely any amount, but at  $40^{\circ}$  one or two grains are dissolved in ten or twelve hours. Ronalds and Eyre, in experiments with regard to the action of water on peat, found that at  $67^{\circ}$  nine grains, and at  $84^{\circ}$  fourteen and a half grains, were dissolved in forty-eight hours; the deduction from these facts being, *ceteris paribus*, that in summer water contains more organic matter than in winter. The atmosphere is a source of these impurities, so that water exposed in tanks, &c., acquires by absorption

the germs of algæ and fungoid vegetation, or even of animalcules, and by the decomposition of matter obtained in this manner becomes vitiated. Light expedites the change, and it is accelerated by stagnation and retarded by motion; hence the difference in purity between stagnant waters and streams. The quantity of organic matter existing in water depends essentially upon circumstances. In streams the percentage of injurious constituents is regulated by several conditions, such as wet or dry seasons, the amount and quality of sewage discharged into them, &c. In wells, this result will be controlled partly by the nature of the strata through which the water passes, but more particularly by the situation, which admits of the infiltration of decomposing or offensive matter into them.

"The organic impurities of water are of two kinds; they occur either in a state of solution or in a solid form. The nature of the latter can be readily ascertained either by the naked eye or by the aid of the microscope, and consists of living or dead animal or vegetable matter. But the former being in a state of solution allows of no such investigation, and, being generally accompanied by the presence of carbonic acid and alkaline nitrates (the result of decomposition), communicates a sparkling appearance and cooling taste, rendering the water deceptively agreeable to the palate. There is a curious circumstance with regard to the production of the different kinds of organisms found in water. If the water is alkaline, infusoria chiefly show themselves; but if acid, fungoid vegetations are more extensively found. This statement admits of verification by experiment; if a small quantity of albumen is mixed with water, in a few days more or less putridity is exhibited, with the development of animal and vegetable life; but if previous to this decomposition the liquid is rendered alkaline, it rapidly putrefies, animalcula are developed, and it speedily becomes in the highest degree offensive. On the other hand, when the water is acidified the impurities will be found to be composed entirely of fungoid vegetation.

"Almost all water contains some organic matter derived from the soil. Even that from granitic districts contains from 0·3 to 0·7 grains per gallon, whilst water which has permeated vegetable soil may afford 12 to 30 or even more grains per gallon."\*

The fact stated above, that, whereas water at 32° Fahr. dissolves scarcely any organic matter, water, at temperatures ranging from 60° to 90°, dissolves it freely, affords a complete explanation why it is that water obtained from the same source all the year round, and which can be drunk with impunity during winter, becomes in summer one of the most active, subtle, and powerful agents of death.

11. *Bad Food and Eating to Excess.*—Bad food, or even good food, in excess and remaining unduly long in the stomach undi-

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\* "The Organic Impurities of Water." By William Proctor, M.D., F.C.S.: *Medical Times and Gazette*, September 8, 1866.



gested, as well as any other irritating substances in the alimentary canal, may induce cholera. "Mr. Grainger, in his Report on the Epidemic of 1848-9, says, 'Several marked examples were brought under my notice, where violent attacks of cholera were distinctly traceable to the use of putrid fish, bad pickled-pork, decayed cheese,' &c. Dr. Carpenter quotes Dr. Brittan as authority for the fact of an outbreak of cholera and choleraic diarrhoea among a number of school children who had eaten plentifully of spoiled oysters, and by which eleven of the sufferers lost their lives. In the earlier days of the appearance of the disease in India, it was thought that many of the attacks were caused by the eating of diseased rice. I remember the case of a gentleman who was attacked a few hours after eating hermetically sealed fish."\* Dr. Barlow, in his remarks upon the pathology of cholera,† observes, "That cholera may be induced by articles of food or other adventitious circumstances is an opinion very generally entertained, not only by the public, but also by medical practitioners." Dr. Macpherson‡ speaks of excess in eating or drinking as an exciting cause: "As instanced," he says, "in H.M.'s 18th Regiment suffering at Christmas time—an unusual season for cholera in Calcutta—or in H.M.'s 14th at Berhampore, after a distribution of prize money. Among natives, abstinence during festivals, followed by an excess of every kind, leads to similar results. And here it may be remarked that most violent cases of vomiting, purging, and cramps simulating cholera have been clearly traced by me and others sometimes to eating bad shell-fish, and sometimes to copper poisoning from the cooking utensils. From the days of Hippocrates downwards, who is very full on the subject, indigestible food has been declared a cause of cholera." He afterwards observes (page 33), that "cholera seizures appear to be most frequent in natives [of India] after a full meal." And again (page 65) that "Beck mentions colic, diarrhoea, and cholera morbus, immediately prevailing, to a great extent, after the arrival of a cargo of bad oysters in New York." Dr. Wood's evidence is to same effect. He says: "Whatever irritates the stomach, may convert a tendency to cholera into its reality. \* \* \* Indigestible food, such as unripe fruit and crude vegetables; excessive eating, even of wholesome food; unwholesome drinks, including impure water, cider, poor wines, and imperfectly fermented liquors of all kinds; very cold drinks, and ices taken too freely, especially when the body is heated; purgative medicines, particularly those of a drastic character; these and many other causes operating in a similar manner are capable of exciting the disease in persons previously exposed to the specific influence."

\* ART. "Epidemic Cholera." By Edward Goodeve, M.B., in "Reynolds's System of Medicine," vol. i. p. 134.

† *Medical Times and Gazette*, August 11th, 1866.

‡ Op. cit., p. 32.



12. *Dental Irritation*.—The process of teething, which by the excessive irritation of the dental nerves, and the consequent hyperæmia of the medulla-oblongata in which they converge, is, in ordinary times, a most fruitful source of diarrhœa, becomes in cholera times, and often in those summers when adults have a complete immunity from cholera, a very active cause of the disease in children. Its co-operation with the extremely vascular condition of the nervous centres in childhood, already adverted to, is an additional reason why, when cholera is epidemic, children form such a disproportionately large number of its victims.

In Chapter III. of the first Part of this book, I have explained the *modus operandi* of bad air, or noxious effluvia; of bad food, or other irritating ingesta; and of dental irritation in causing diarrhœa: when they induce cholera, they do so precisely in the same manner. When other causes are co-operating to constituting a predisposition to cholera, those now in question, though active only to the extent which in ordinary times would result in mere diarrhœa, are now adequate to originate the more formidable disease. On the other hand, if the influences conducive to epidemic cholera are not present, extremely energetic action of noxious gases, putrid water, irritating ingesta of other kinds, or excessive nervous excitement occasioned by teething, are capable of bringing on attacks of cholera.

13. *Purgative Medicines*.—That these are capable of inducing cholera when the disease is epidemic is proved by an amount of evidence from the most competent and impartial observers, placing the fact wholly beyond question. As the truth in this matter is in its bearings on treatment of paramount importance, I shall cite several authorities concerning it.

"The number of instances," says Sir Ranald Martin, "which I observed in Calcutta of Epsom or Seidlitz salts leading to profuse serous diarrhœa, and subsequent fatal cholera, is a circumstance worthy of notice. The dose was always taken in the early morning, and the patient had gone through his ordinary avocations throughout the day, mistaking for the operation of a saline aperient what was in reality the induction of choleraic diarrhœa, true cholera, and deadly collapse. In the end of March, 1833, I was requested to visit the head partner of a large house of business—a healthy and powerful man. The note, which was not expressed with any urgency, was delivered to me at 5 P.M., near to the house of the patient, so that I was with him within a few minutes of its being written. I found the gentleman at his desk at work. His countenance and voice at once expressed fatal collapse, and he died at eight o'clock that evening. He stated that at five o'clock that morning he had taken a dose of Seidlitz, his health previously having been quite good."

Referring to a form of epidemic fever which visited Calcutta, 1834, and which was termed the "cholera fever," the same author

says,—“So mild a fever I seldom recollect to have seen, yet it was most dangerous to treat—the irritable condition of the bowels, which formed so prominent a symptom of the epidemic, being readily aggravated into fatal cholera whenever purgatives of a saline or drastic nature were exhibited. At first the peculiar character of the fever was not known; and I remember hearing at the time that several deaths occurred where strong purgatives had been exhibited *over night*; indeed, one such case came accidentally under my observation. \* \* \* Others fell victims, in like manner, to the hasty exhibition of purgatives. \* \* \* Not only was the peculiar fever here noticed prone to merge into true cholera, but I had to remark, on three several occasions in Calcutta, that when the cholera poison was present in a concentrated form, we were for the time precluded from treating the cases of ordinary remittent fever of Europeans in our usual manner.”\*

Dr. Macpherson, whose great experience of cholera in India equals that of Sir Ranald Martin, ranks “excessive action of the bowels from purgative medicines” among the predisposing causes of cholera, and states that “a dread of purgatives had been generally accepted by practitioners at Calcutta, though possibly this dread, especially of saline aperients, may be somewhat exaggerated.” Referring to sporadic cases, he says,—“While the hot winds were blowing as strongly as they ever do in Calcutta, a retired subordinate officer, living in the General Hospital, was seized. He had taken an aperient that he was accustomed to use, and to this he ascribed the attack of which he died.” And again, referring to a remarkable sporadic outbreak of four cases, three of which were fatal, he says,—“They unfortunately had aperient medicine given them, and no purgative is safe at such a time.” Dr. Laycock writes of the premonitory stage of cholera:—“Charge your patient, as he values his life, not to irritate his gastro-intestinal mucous membrane;” and Dr. Mackintosh, reviewing the results of the very interesting experiments made at the Drummond Street Hospital, says,—“I cannot state the fact too strongly, that purgatives are dangerous remedies.” Twining alludes to the bad effect of giving purgatives too early when recovery is taking place, and I have seen some of the worst relapses follow the too early use of castor oil. \* \* \* The misapplication of purgatives is often irremediable.”† Speaking of the “noxious effects of purgatives, given during cholera,” Dr. Goodeve, who believes that these effects are “not confined to the saline and hydragogue purgatives only,” says, “I have seen milder purgatives followed by cholera.” Dr. Painter says, “I would firmly express the opinion, that diarrhoea, of

\* “The Influence of Tropical Climates in producing the Acute Endemic Diseases of Europeans,” pp. 556, 557.

† “Cholera in its Home,” pp. 13, 32, 35, 38, 108-9.

whatever kind, in cholera seasons, should be carefully checked, and that persons should not even take aperients of their own accord. I believe that a relaxed state of the bowels, however brought on, tends, in cholera times, to render the subject of it a fit recipient for that disease."\* "Dr. Durham [of Newcastle-on-Tyne] records the case of a seaman admitted into hospital for concussion of the brain, from which he was recovering; but, being very costive, he was ordered half an ounce of castor oil. Cholera supervened, and he died in a few hours."† Dr. Barlow, in his paper already mentioned, says, "That cholera has been hurried on by aperients, and possibly a fatal result determined by their instrumentality, certain facts that have come under my observation leave me little room to doubt."

In short we find, as Dr. Macpherson has concisely stated, that "the great majority of writers in all countries pronouncing their opinion, that when cholera is prevalent it is not safe to take aperients. Twining, Martin, Morehead, nearly all writers agree in this. It has been the common doctrine in Europe, and the latest experience in France confirms it. The very fact that patients so often attribute their attack to their use of purgatives, whether or not they do so on sufficient grounds, is at least enough to prove that purgatives do not avert cholera, and is not very encouraging to their employment" (pp. 107-8).

The *modus operandi* of purgative medicines is, I presume, now generally recognised as being through the agency of the nervous system, even in those cases in which the medicine enters the circulation. If this be the case, whether the purgative acts primarily on the peripheral extremities of the intestinal nerves, and produces excessive activity of the bowels by "reflex action,"—whether the nervous centres presiding over the bowels are excited to excessive activity by a change in the blood circulating in them—or finally, whether, as in the case of calomel, the liver is also specially stimulated to excrete an undue quantity of bile, which may irritate the intestinal mucous membrane,—the enteric nervous centres are, in any case, unduly excited, have an excessive afflux of blood, and thus become preternaturally energetic. Now this is precisely one of the conditions which obtains during an attack of cholera, and which, when the causes already mentioned are exerting their influence, is likely to induce the disease. And, conversely, when by the action of purgatives, the functions of the enteric nervous centres have become intensified, the hyperæmic condition of those centres only needs the co-operative influence of either excessive heat, or some one of the other epidemic agencies to ensure the development of the disease. A consideration of the *modus operandi*

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\* "Remarks on the Nature and Treatment of Cholera." By R. B. Painter, M.D., F.R.C.S., published in the *Lancet*, July 28th, 1866.

† *Lancet*, August 25th, 1866.

of purgative medicines here indicated, in connexion with the pathology of cholera already expounded, explains at once why, in cholera times, their use is so beset with danger, and how not unfrequently they become potent causes of the disease itself.

14. *Opium*.—The administration of this drug to cholera patients is now generally admitted to be attended with danger if it be given after collapse has set in. Professor Maclean, after hearing my lecture on cholera at Southampton, referred to my remarks respecting the dangerous effects of opium, and said "that he could bear the strongest testimony not only to the inefficacy, but to the very great danger in the use of opium in cholera;"\* and in a letter to Dr. Aitkin,† he says: "Opium in cholera should be given only in the premonitory diarrhœa. At this stage, in combination with a stimulant it is often of the highest value. If persevered in, particularly in strong doses, it is a dangerous remedy, inducing fatal narcotism, or at the least, interfering with the functions of the kidneys, and so leading directly to uræmic poisoning. \* \* \* In the stage of reaction \* \* \* suppression of the secretion of urine is most to be dreaded where opium has been too freely used in the treatment." Dr. Goodeve expresses a somewhat similar opinion: "When the collapse is progressive no opium should be given after the third grain, and indeed if it seems to be approaching fast even the third grain should be withheld. When collapse is complete, opium should be entirely avoided." Dr. Macpherson's advice is to the same effect: "The moment it becomes plain that the disease cannot be arrested in its early stage, the opium is to be discontinued" (p. 115).

The medical journals contain, scattered through their pages, numerous reports of experience of the power of opium to favour or induce the development of cholera after the disease has fairly set in, and of its failure in a large number of cases to arrest the premonitory diarrhœa. Now, no physician doubts the fact that opium exerts a specific influence on the nervous centres, an influence, the effects of which, so far as the brain is concerned, consist first in exaltation and then in depression of the activity of the cerebral functions. Therefore when co-operating with other causes, or when acting alone in the production of choleraic collapse, it does so, as will be generally admitted, by virtue of its great and peculiar power of modifying the vascular and functional condition of both the cerebro-spinal and sympathetic nervous centres. The pathology of cholera already expounded reveals for the first time the modus operandi of opium in developing the disease; while the admitted facts that opium has often induced collapse; that collapse so induced is more than ordinarily fatal; and that when reaction suc-

\* Report of Meeting of South Hants Medico-Chirurgical and Microscopical Society in the *Hampshire Independent*, Oct. 7, 1865.

† "Science and Practice of Medicine," vol. i. p. 658.

ceeds to it, that reaction is often unusually protracted and dangerous, constitute additional evidence of the truth of the pathology in question. But indeed the subject of the influence of opium in cases of cholera is one of such extreme importance, and, so far as I know, is not yet understood, that I think it necessary to discuss it *in extenso*, and therefore while adverting in this chapter to opium as one of the causes of cholera, shall reserve an ample discussion of it for the section on the *medicinal* treatment of cholera, which will form a part of the next chapter.

15. *The influence of Alcoholic Fluids in producing Cholera.*—It was found by Dr. Farr\* that “on Saturday, Monday, Tuesday, and Wednesday, the deaths from cholera were above, and on Thursday, Friday, and Sunday, below the average. In the whole country Tuesday was the most, Friday the least, fatal day of the week. The disparity in the deaths was the greatest in London, where 2194 persons died on Mondays, 2136 on Tuesdays, and only 1927 on Thursdays, and 1824 on Fridays. The distribution of the deaths over the days of the week is shown below in the table.

In reading the table it must be recollected that the days given are the days in which deaths *occurred*, and that half the deaths happen in the twenty-four hours after the *attack*.

The weekly wages are generally paid on the Saturdays, and the Mondays in London and other cities are days on which a certain proportion of the population indulge in intoxicating drinks. The Fridays are days of comparative abstinence.

	Mondays.	Tuesdays.	Wednesdays.	Thursdays.	Fridays.	Saturdays.	Sundays.	Average.
All England and } Wales . . . . }	7693	7826	7621	7607	7167	7769	7610	7614
London . . . .	2194	2136	1978	1927	1824	2067	2011	2020
Other parts of Eng- } land and Wales . }	5499	5690	5643	5680	5343	5702	5599	5594
All England and } Wales . . . . }	+ 79	+212	+ 7	—7	—447	+155	—4	
London . . . .	+174	+116	— 42	—93	—196	+ 47	—9	
Other parts of Eng- } land and Wales . }	— 95	+ 96	+ 49	+86	—251	+108	+5	

NOTE.—The *minus* sign (—) denotes *below*; the *plus* sign (+) *above* the average.

The remarkable increase of deaths on Tuesday is in striking accor-

\* “Report on the Mortality of Cholera in England, 1848—49.”



dance with the fact that Monday\* (*Saint Monday*, as it is often called by the working classes) is the day especially devoted to idleness and drinking. As observed by Dr. Farr: "The exemption of Hertfordshire from cholera is evidence that *cider*, notwithstanding the popular prejudice, is a much safer beverage in the time of an epidemic than the spirits which were so largely consumed during the time of cholera in the places where it was most fatal." The comparatively great exemption of cider-drinkers has, I am told, been also observed in France. The greater liability to cholera of the intemperate is noticed by almost all writers on the disease; in the epidemic of this year it has been noticed both in Berlin and London. The mortality in Berlin suddenly rose on certain days, and was clearly referable to excesses in drinking; and Dr. Andrew Clark has stated recently, in respect to the London Hospital, "that immediately after pay-day among workmen there had been a great influx of cholera patients." These sudden manifestations of the effects of even one day's drinking are established facts; so that alcohol taken habitually, or only on special occasions, is proved to be an adequate cause of cholera. The blood-poison and zymotic theories of the disease afford no explanation of this fact. How alcohol can, as Sir Ranald Martin suggests, "favour the dissemination, and probably the generation, of the cholera poison," it is difficult to imagine; but just as in respect to opium, the pathological doctrine I have put forward at once explains and is confirmed by the fact in question: the special affinity of alcohol for the nervous centres, its great exciting influence on those centres, and its power of producing vomiting and purging in many cases, when taken in considerable quantities, are well-ascertained facts; it therefore not only produces, in an especial degree, hyperæmia of the nervous centres, and thus, according to my doctrine, an especial predisposition to cholera, but often also two of its leading symptoms.

16. *Habitual Excitants of the Nervous System*.—All influences—whether intellectual, moral, or physical—which tend to exalt the activity or susceptibility of the nervous system, increase its liability to derangement, and notably predispose it to those perverted actions called "functional diseases," of which, as I maintain, diarrhœa and cholera are examples. It seems to me probable that among the physical agencies in question, not only opium and alcohol, but coffee and tea stand conspicuous. I have already referred to the special influence of opium when given as a medicine, and alcohol when taken in considerable quantities, in developing cholera; but the prolonged use of nervine stimulants as personal or national habits are likely to induce such constitutional modifications as to render the system more "nervous," and thus to cause its

\* At a meeting of the Mansion House Cholera Relief Committee. See *British Medical Journal*, Sept. 8, 1866.

diseases to change their type. I believe that the large amount of opium consumed in Eastern countries, and especially in India, creates in the nervous system a condition peculiarly favourable for the generation of cholera by the solar heat; that this condition, though in a lesser degree, is also induced by the habitual consumption of alcoholic fluids; and that the increase of cholera in Europe and America within the present century may be referred not only to the increased activity of the nervous system as expressed in the intense excitements, anxieties, and struggles incident to the present phase of civilization, but also to the free indulgence by the people in nervous stimulants of various kinds, including opium chewing, alcoholic drinks, and, especially during the present century, to the daily consumption in large quantities of coffee and tea. I have a strong conviction that coffee is capable of acting very injuriously on persons predisposed to undue action of the automatic nervous centres. I know that in some cases it exerts a peculiar and distressingly exciting influence on the heart; and as its effect on the kidneys is, like opium, that of a stimulant, I presume it excites the spinal cord and sympathetic ganglia somewhat as opium does. Moreover, I have been informed of one case in which a cup of coffee acts as a purgative; and it is customary for the Italian peasants, at least in the districts around the lake of Como, to administer strong coffee containing lemon-juice in cases of dysentery. They have great faith in the efficacy of the remedy. If coffee does act curatively in dysentery, the fact confirms my view of its mode of action; for dysentery is an inflammation, and stimulation of the enteric nervous ganglia will cause contraction of the mesenteric arteries, and thus to lessen the supply of the blood to the bowels. Tea is, I incline to think, less objectionable; but its power of rapidly inducing perspiration has long been recognised, and its blessed influence in this respect has long been used to lessen the heat of and to induce moisture in the skin of feverish patients. What I have said in the Introduction concerning the physiology of sweating considered in connexion with the fact that tea will induce perspiration, points to the conclusion that at all events it induces hyperæmia of the automatic nervous centres—the condition favourable for the development of cholera. It is well known that the effects of tea and coffee, if taken in excess, are cumulative, so that they do not exert less but more power over those who take them as years advance. Many persons, who at twenty sleep soundly after drinking freely of tea or coffee, are obliged when they become twice as old to forego the gratification of taking either of them freely in the evening, because they know a sleepless night would be the consequence. This fact is a striking proof that during even a lifetime constitutional conditions may be so modified as to generate a predisposition to diseases, to which the system was not previously liable, and to cause diseases themselves to change their type.

I may add here that another powerfully *predisposing* cause of



cholera consists, I am inclined to believe, in immoderate indulgence of the sexual appetite, and in all perverted excitations of the reproductive organs. It is well known that undue sexual excitement conduces to the development of diseases of the nervous system, especially to that disease in the action of which the whole system is involved—viz., epilepsy. A chronic congestion or preternatural excitability of the spinal cord is established; and in this condition, assuming my hypothesis as to the nature of cholera be true, there exists a special aptitude for the development of that disease by the solar heat. As Asiatic races are peculiarly prone to sexual intemperance, it is not unlikely that this is one of the causes of the frightful destructiveness of the disease amongst them, and especially in India.

17. *Mental Emotion.*—In the first part of this volume I have adverted to the power of emotion as a cause of diarrhoea, and have given several instances of its operation in this respect. That fear, fright, and panic are fruitful sources of cholera is attested by a large number of authoritative witnesses. There is an old, and often repeated story of an encounter outside an Eastern city between the plague demon, when about to enter the city, and a citizen, who asked what he was going to do there, and who was told by the demon that he was going to kill 3000 people. On his return from the city, the same citizen taxed him with lying, inasmuch as 30,000 had been destroyed. True, said the demon; but I only killed 3000; fear killed the rest. This story contains a great truth, which is recognised by almost every observer of a cholera epidemic. Referring to the last epidemic in Turkey, Mr. Harry Leach says,—“Panic undoubtedly increased its intensity in many instances, and brought cases of simple diarrhoea into the grip of cholera very speedily indeed.”\* Drs. Bell and Stokes† remark, “Many have been destroyed by fear alone;” and Dr. Wood‡ says, “Sudden and strong emotions, often bring on an attack.” Dr. Macpherson observes, “Cholera attacks all who are depressed. \* \* \* Those who are most alarmed are most likely to be seized, and confidence seems to be of use both in warding off an attack and in struggling through it. The excessive alarms during epidemics are most injurious” (pp. 31–3). Dr. MacCormack § says he can assert from personal experience that “fear alone will produce the disease,” and makes the following statement (often made also by others) for which, however, he gives no authority. “In the year 1832, a man was unfortunately tempted, by a large sum, to occupy for a certain time a bed in which he was

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\* “Brief Notes on the Last Epidemic of Cholera in Turkey,” p. 9.

† Op. cit., vol. i. p. 370.

‡ Op. cit., vol. i. p. 689.

§ “Cholera: its Causes, Prevention, Non-Contagiousness, and Treatment,” p. 48.

informed a cholera patient had died; and although such had not been the case, he nevertheless from pure fear, was in a very short time seized with the symptoms of cholera and died." Dr. Forbes Winslow pithily remarks, "During an attack of cholera, the patient who has the least fear of dying has, *ceteris paribus*, the best chance of living."\* Can these facts be accounted for by the organic-poison hypothesis? Its advocates will perhaps say,—“Yes, fear predisposes to the reception of all zymotic diseases.” I believe it does; but, it must still be admitted that fear predisposes to an attack of cholera far more strongly than it does to any disease of an indubitably zymotic character, and the problem for solution still remains therefore essentially the same, and hitherto insoluble. But the pathology of cholera now expressed solves that problem completely. The *modus operandi* of terror, and other strong mental emotions in producing diarrhœa, is explained in Chapter IV., Part I.; the same explanation applies to cholera, only that, *ceteris paribus*, the emotion productive of cholera must be more intense than that which suffices to produce diarrhœa. But during very hot weather, or when the specific atmospheric condition causing epidemic cholera prevails, fright, terror, or other mental emotion only so intense as in ordinary times to be productive of diarrhœa, will then produce the more formidable disease.

18. *Influences Chiefly Operative during the Night.*—It has been noted by many observers that cholera begins most frequently during the night, and especially between two and four o'clock in the morning. "The worst cases of cholera, especially at the commencement of the epidemic, occurred much in this way:—The patient went to bed apparently in good health, and was awakened about two or three in the morning with weight and pain across the epigastrium, and desire to relieve the bowels. Their action would be followed by considerable relief to the patient. He would return to bed, but in a short time another and much more liquid evacuation would occur; after which he would feel exhausted and alarmed.† In tracing the origin of the cases which I have attended, I have found that a large proportion of them begin after midnight, and before four A.M. I have also observed that the disease, when not already in the stage of collapse, is more liable to increase in severity, and that relapses are most frequent, during the same hours. More abundant and precise observations on this point are much needed; but I have no doubt that the fact in question is characteristic of cholera, and that it is one of important pathological significance. Perhaps what I am about to say will invest it with sufficient interest to induce physicians to note exactly, and to

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\* "The Depressing Passions in Relation to Cholera." *Pall Mall Gazette*, August 25th, 1866.

† "Remarks on the Pathology of Cholera." By G. H. Barlow, M.D.: *Medical Times and Gazette*, August 11th, 1866.

publish in their reports of cases, in what part of the twenty-four hours attacks of cholera most usually come on, or increase in severity, and also when relapses are most liable to occur.

As it appears to be already established that the three or four hours immediately after midnight are the periods of greatest danger, the question arises, what is the reason of this fact? During the hours in question the temperature is lower than during any other of the twenty-four. Moreover, though cholera is most destructive in summer, the greatest number of deaths generally occurs in Europe during August and September, when the alternation of temperature within each twenty-four hours is often considerable, the days being hot, and the nights cold. Inasmuch as the number of attacks is greatest during those hours when the temperature is lowest, it seems not improbable that a diminution in the circulation of the blood in the surface of the body, and in the pulmonary mucous membrane is induced by these causes, and that this condition favours the access of diarrhoea and cholera. It will perhaps be objected that though the temperature of the air may be lowest during the hours in question, the surface of the body is kept at least as warm by the surrounding bed-clothes as it is during the day. This is undoubtedly the case with respect to the upper and middle-classes, few members of which, however, are attacked with cholera; but precisely the classes which furnish the great majority of the victims of this pestilence are those who are most inadequately supplied with suitable bed-clothing, and who are therefore exposed in the most extreme degree to whatever baneful influence may be exerted by falls of the atmospheric temperature during the night. No one who goes much amongst the poor and examines their bedding and bed-clothes can fail to be impressed with the conviction, that every night, when the temperature is especially low, they must suffer greatly from cold. In so far as they do so, a predisposition to cholera is established. Moreover, while inadequate clothing thus permits the surface of the body to become cool, the cold air inspired will co-operate in the same direction; but, even though the body be well clothed, breathing the cold air must tend to effect some contraction of the bronchial tubes, and in as far as it does so, diminishes the oxygenation and circulation of the blood, and thus constitutes to the extent of the diminution a predisposition to cholera. The overcrowding and bad ventilation of the dwellings of the poor also operate in impeding the healthy oxygenation of the blood; and precisely when the temperature is lowest, the ventilation is least, and the breathing of exhaled animal effluvia most—conditions especially co-operative with that of the sudden advent of atmospheric cold in favouring the invasion of cholera.

But if the pathological doctrine explained in this chapter be true, the chief reason why cholera attacks its victims most frequently during the early morning is, because they are then in the

most profound sleep—a physiological state in which the vascular condition of the whole nervous system during health approaches most nearly to that existing in cholera patients. When advert- ing to the state of the brain of such patients, I mentioned that during normal sleep there is less blood in the brain than during wakefulness. It is also well-established that during normal sleep the vital processes, or the textural changes throughout the body, are at their minimum degree of activity: respiration, oxy- genation, and circulation are all diminished; therefore the amount of carbonic acid expired is lessened, and the temperature of the surface of the body falls to an appreciable degree. Moreover, in many cases, and I believe precisely in those in which sleep is most healthy, there is free, but not excessive perspiration, the skin being suffused with what is felt to be a healthy moisture. The profuse sweating of consumptive patients is, as I am prepared to show, only an excess of this attribute of normal sleep. Now, the diminution of blood in the brain, and in the surface of the body, and the diminution of respiration as well as the increase of perspiration, are all evidence of, and produced by, an increase in the supply of blood to the sympathetic ganglia and spinal cord; and in persons remarkable for sleeping healthily, and for passing from wakefulness to sleep easily, each of these phenomena, perspi- ration included, may be observed within a few minutes from the time they fall asleep—a fact which fairly proves that they are imme- diately due to a rapid and decisive increase of blood in the nervous centres in question. It thus appears that the state of sleep in- volves anæmia of the brain, and hyperæmia of the sympathetic and of the spinal cord. This condition of the nervous centres, when excessive, is, as I have shown, the proximate cause of cholera; and therefore, while it obtains in a lesser degree during sleep, they are susceptible in a maximum degree of those influences which originate cholera. Hence it is that this disease, as well as epilepsy, is most prone to surprise its victims in the night. The perfect explanation which my hypothesis affords of this recognised fact is another evidence of its truth.

19. *The Tendency of Cholera to recur in the same Individual and in the same Nation.*—The whole body of evidence presented in this and the previous chapter supports the doctrine, that the agencies pro- ductive of cholera have no affinity with what is generally under- stood as an organic or blood-poison; that the operation of, at least, nearly all of them, is of a dynamic character, and that the result- ing disease consists primarily in a dynamic derangement of the circulation of the blood in the automatic nervous centres, and secondarily, in a like derangement caused by the primary one in all the other organs of the body. Assuming the correctness of this conclusion, we are necessarily lead to perceive, what indeed has been already suggested, that while there exists on the one hand a certain

essential affinity between cholera and ague, there is, on the other hand, a not less striking analogy between cholera and epilepsy. The distinguishing feature of ague is the recurrence of its paroxysms at certain periods, generally so regular that the several forms of the disease derive their epithets from the times when the fits recur; thus, there are *quotidian*, *tertian*, *quartan*, *tertiana duplex*, and *quartana duplex* paroxysms, as well as modifications of these called *postponing* and *anticipating* fits. As the disease declines, the fits commonly recur at less and less regular intervals, but when ague has once been established in the system, its tendency to recur at different periods throughout life is one among many of its remarkable attributes. So remarkable indeed is it, that Sir Thomas Watson, when speaking of the predisposing causes of the disease, says—"The strongest predisposing cause of all is an actual occurrence of the disease itself. The effects of former intermittents upon the system is such, that the complaint may be reproduced by agencies which, under any other circumstances, would be quite inoperative in exciting ague. I have stated already my persuasion that, strictly speaking, there is but one exciting cause of intermittent fever; but in making that statement I refer to its *first production*. The disease leaves the body in a condition in which other injurious influences may, of themselves, be sufficient to renew it. It brings into play a new order of exciting, or rather re-exciting causes. If a person were never exposed to the malaria, he would never, I believe, have ague; but having once had ague, he may many times have it again, although he should never again be subjected to the direct influence of the malaria." Sir Thomas Watson mentions a case of a gentleman who was attacked while in the West Indies with remittent fever, which "assumed by degrees a distinctly intermittent form, and became a tertian; and at last he got well, and strong, and came over to this country. Yet for a long while, though to all appearance his health was re-established, ague-fits would from time to time occur, and they came precisely at the day and hour on which they would have happened if the tertian had continued with its original type, and slight causes were sufficient to reproduce them. He had marked, in an almanack, the days of the expected accession; and on those days it recurred, for some time, whenever the *east wind blew*. This very circumstance, the east wind, is a *common* re-exciting cause in such cases; exposure to cold in any way is another."\*

My interpretation of these very remarkable phenomena is as follows:—The malarious influence operating upon the large nervous expanse of the pulmonary mucous membrane stimulates powerfully the vaso-motor nerve centres; which, therefore, become excessively

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\* "Lectures on the Principles and Practice of Physic," 4th edition, vol. i. pp. 744, 745.

hyperæmic, and being so, their reflex actions on the whole arterial system are necessarily correspondingly vigorous and intense. Soon, however, the extreme energy of these ganglia is succeeded by exhaustion, with a corresponding change in their vascular condition, and hence their tonic influence over the systemic and pulmonary arteries declines to a minimum. During their maximum of energetic action, the arteries throughout the body are contracted, and hence the rigor of ague; and, again, during their succeeding exhaustion, the arteries throughout the body being relaxed, dilate extremely, the blood is admitted to all the organic tissues with undue copiousness, and hence the hot stage. The extreme contraction and dilatation of the delicate muscular walls of the minute arteries pervading the nervous centres, once effected, establishes in them an excessive or abnormal mobility, and hence predisposes them to contract and dilate unduly when exposed to stimulant influences which previously were too feeble to exert any notable power over them. Thus it is that the mysterious tendency of organic phenomena, as for example, menstruation, to recur periodically at regular intervals, manifests itself in the alternate force and weakness of the sympathetic ganglia, and the consequent cold and hot stages of intermittent fever. After the disease in its active form has passed away, the preternatural excitability which it has established in the nervous centres remains, and hence it is, that though the person may be afterwards far removed from the original exciting cause of the disease, he carries within himself, in the form of the acquired preternatural excitability of the sympathetic ganglia, and notably of the preternatural mobility of the coats of their blood-vessels, a predisposing cause of the disease, which may now be called into activity by a variety of slight exciting causes—the east wind, for example.

In like manner, but in a far more striking degree, the tendency of epileptic fits to recur, is one of the most characteristic phenomena of the disease. When the first fit happens during adult life, it is almost sure to be the precursor of a series of fits; and even if the disease first appears in the guise of infantile convulsions, when the recuperative force of the nervous system, as well as of the rest of the body, is at its maximum, such is the profound impression imprinted on the organism by these attacks, that notwithstanding the exuberant vitality of childhood and the consequent tendency of the body during the first years of life to “outgrow” its diseases, epilepsy breaks out later in life in a considerable proportion of those cases in which infantile convulsions have occurred, and every succeeding fit does but intensify the predisposition to their constant recurrence.

If this doctrine of the proximate cause of the tendency of ague and epileptic fits to recur be true, and if the affinity which I have indicated between these diseases and cholera, really exists, the conclusion seems inevitable, that a person who has once had cholera is, *ceteris paribus*, more likely to have it again than one who has never had it at all; and in the same way as the tendency to epilepsy be-

comes hereditary by the establishment of preternatural mobility in the vascular system, so the children of those who have suffered from cholera have a hereditary predisposition to become victims of the disease. Hence it is that, co-operating with many other of the influences now reviewed, this predisposition tends to render the disease, which at one time had been almost exclusively epidemic, a more or less permanent endemic of the country where it has appeared.

It is well known that an attack of cholera confers no security against its future invasions. "Previous attacks," says Dr. Goodeve,\* "do not appear to confer any immunity: there are numerous cases on record of persons who have had cholera more than once." Dr. Macpherson observes:† "No previous attack of it gives security against this pestilence;" and Professor Parkes remarks:‡ "Certainly there is a peculiar constitution, or perhaps condition of the blood, which predisposes to cholera. The following case seems to prove this: In 1843, a soldier was admitted with slight but well-marked symptoms of algid cholera, from which he rallied without consecutive fever. In 1845 this man was apparently in perfect health: he was stout and robust, of remarkable quiet habits, and a teetotaller. He was the second man in the regiment who was attacked with the disease, and immediately recognised the nature of the complaint, from his recollection of the symptoms he had experienced two years before. Now in this case, which proved fatal, was there not some peculiar condition of body, which rendered this man more obnoxious than almost any other of the 1000 men in the same regiment to the action of the virus?"

The considerations here advanced are in remarkable accordance with the history of cholera, which shows its tendency to become more and more widely spread, to recur as an epidemic at increasingly frequent intervals, and, finally, when not becoming endemic, as it now is in India, to establish itself as a regular summer visitant of countries within the temperate zones.

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\* ART. "Epidemic Cholera" in Reynolds's "System of Medicine," p. 139.

† "Cholera in its Home," p. 33.      ‡ Researches, &c., pp. 197, 198.





## CHAPTER V.

### TREATMENT.

#### SECTION I.

#### *Treatment by Cold and Heat.*

IF the hypothesis now expounded be correct, it is obvious that the whole aim in the treatment of cholera should be the restoration of the circulation of the blood to its normal state of equilibrium in all parts of the body, and primarily and pre-eminently in the automatic nervous centres. Whatever agents will most rapidly and completely effect this result will prove the best remedies for cholera. When the truth is once thoroughly established, that all the phenomena of cholera proceed from hyperæmia of these centres, the attention of physicians will be earnestly engaged in solving the problem, What is the best means of restoring the circulation to its normal equilibrium, and especially of subduing the hyperæmia of the spinal cord and sympathetic ganglia? I shall not be surprised if a method is discovered of applying electricity or galvanism, so as to accomplish the object in view.

In the *Lancet* of August 12, 1865, Dr. Althaus states that "there are facts to prove that the continuous [galvanic] current has a physiological action on the spinal cord and the sympathetic, if applied to the skin of the back by moistened conductors. Thus, we may often cause the iris to contract by directing a current of large quantity to the lower cervical and upper dorsal vertebræ, showing that there is physiological transmission of part of the current to the cilio-spinal region of the cord and the corresponding ganglia of the sympathetic which preside over the functions of the iris. Again, by applying a continuous current to the lumbar portion of the spine we may cause a glow in the legs and feet, without any direct application to these latter, showing that the influence on animal temperature, which M. Claude Bernard and Dr. Brown-Séquard have proved to belong to the sympathetic, is brought into play by the application of the continuous current." Dr. Althaus's discovery seems to be confirmed by a French investigator, for I have received from a friend in Paris a letter, dated August 3, 1866, containing the following question:—"Have you seen anything of M. Lefort's discovery (?) of galvanizing the spine in cases of syncope?"

If the interesting facts here announced should be verified by

further experience, the application of the direct or galvanic current to the spine will exert the same kind of influence on the spinal cord and sympathetic, and through the latter on the vascular system, as is exerted by the application of ice. The therapeutical consequences of the discovery, if confirmed—as it seems, *à priori*, not unlikely to be—will doubtless prove important. Of course, a galvanic battery is much less available and manageable than a spinal ice-bag; but it may be that in cases of cholera, where ice is not obtainable and a galvanic battery is, the disease may be at least controlled and perhaps cured by the latter.

I make these remarks with full knowledge that galvanism and electricity have been tried in several cases, and have failed, although in a few rare instances galvanism has exercised a markedly beneficial effect—recovery having followed its use. When these attempts were made, the two conditions which are absolutely essential to insure success were not known to be necessary. These are: *first*, that the continuous current only be used; and, *second*, that its application be made to the whole spinal region, and to that alone.

In the present state of knowledge there is, however, no available power of subduing hyperæmia of the automatic nervous centres comparable to that of ice applied along the spine. It has been shown already in Part I., that that amount of hyperæmia in question which results in the various forms of diarrhœa already discussed can be effectually subdued by the spinal ice-bag. Moreover, the great leading symptoms of cholera—vomiting, purging, cramps, and coldness—when met with separately as symptoms of other diseases, are capable of being subdued in the majority of cases by means of ice along the whole or some part of the spine. I have already given ample evidence that the vomiting of sea-sickness can be thus cured. I possess evidence of a satisfactory character, that the vomiting incident to pregnancy can also be arrested in the same way; and I have had extensive experience that vomiting symptomatic of various diseases, including cerebral, gastric, and uterine disorders is not less amenable to this curative method. Its power over diarrhœa has already been shown. It has been tried in a great number of cases of muscular disorder, in which cramps and spasms were the distinguishing features, and has been found an incomparably more effectual remedy than any other known; and in cases of the tonic spasms and tetanic rigidity sometimes accompanying sea-sickness, its power of completely abolishing those morbid states has been fully established. I can state from my own experience, that in a great number of cases of coldness of the surface of the body occasioned by arterial spasms, normal circulation and heat have been restored by the application of ice along the spine. In the Introduction I have stated and explained the mode of action of both cold and heat thus applied, and have mentioned numerous remedial effects of their application; but as especially bearing on the therapeutical power of ice along

the spine in cholera I refer the reader to my reports\* of cases of sea-sickness cured by this method; for they prove that not only was the vomiting, as well as the diarrhoea and spasms, when present, arrested, but the cold sweat was also completely stopped, and the patient who was previously pallid and cold quickly recovered his natural colour and warmth. The case described by Dr. Lee, and reported at the end of Chapter III., affords striking evidence that vomiting, tonic convulsions, cold sweat, and coldness of the surface of the body are completely remediable by the spinal ice-bag. I repeat the doctor's words:—"The effects of its application were little short of miraculous. In three minutes the retching ceased and the spasms were calmed. In a quarter of an hour she had fallen into a quiet sleep; and in half an hour her hands and feet were of natural warmth, and her face had regained its wonted colour." A case of that frightful and generally fatal form of tonic convulsions—tetanus—was cured some months ago in Yorkshire by means of the spinal ice-bag after everything else had been tried in vain. Another case cured in the same way is reported in *The Australian Medical Journal* for March, 1866; chloroform and atropine, three doses of each, were given in this case; but no one reading the report of it will fail to be convinced that it was the ice along the spine which saved the patient. "In two hours after its application the spasms had considerably and decidedly diminished. \* \* \* Our entering the room caused such an excitement in the patient, as to bring on all the symptoms that had been abeyance for four hours and a half. The application of the ice was renewed, and in twenty minutes the spasms had disappeared." In the *Medical Times and Gazette* for 1866 (I forget the number) there is a report of a case of tetanus treated with ice, which *subdued the spasms*, but did not effect a cure: the patient died, and the post-mortem examination revealed *strumous* inflammation of the spinal cord, which, of course, nothing could cure.

So assured am I of the power of ice, when properly applied, to subdue hyperæmia, extending even to inflammation of the spinal cord, that, when the Czarowitch was suffering from cerebro-spinal meningitis, to which he became a victim, I made the utmost effort (by communicating with the Russian Ambassador in London, and otherwise), to cause this method of treatment to be brought under the consideration of the distinguished patient's physicians. As was to be expected, my representations were unheeded; but that the conviction which dictated them was well founded, has now been thoroughly proved by an extensive experience in treating the recent epidemic disease—cerebro-spinal meningitis—by the external application of ice. The favourable results of this method have already been affirmed in an official report issued from the Medical Depart-

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\* See my pamphlet on "Sea-sickness."

ment of the Privy Council; and in the elaborate and exhaustive monograph on the disease by Dr. Hirsch, he has given the most emphatic testimony to the efficacy of this treatment, as will be seen by the extract from his work in the subjoined note.\*

Having proved that each of the formidable symptoms of cholera, when occurring in other diseases, may be overcome by means of the spinal ice-bag, I have, I think, fairly shown an *à priori* probability that when these symptoms occur together, the application of ice along the spine is the most powerful, as well as the most scientific remedy which can be had recourse to. The experience which I have already had certainly justifies this conclusion in a very encouraging degree, as will be seen by the statement of facts in the next chapter.

But an extremely important co-operative remedy consists in the simultaneous application of heat to the general surface of the body. I am aware that heat when thus applied, by whatever method, has generally failed to recover patients from collapse, except in favourable cases, in most of which recovery would have occurred without any treatment at all. The reason of this is, that heat so applied does not, and cannot, affect the condition of the blood-vessels ramifying throughout the spinal and sympathetic nervous centres in any considerable degree. Hence the hyperæmia of those centres being allowed to persist, the arterial spasms throughout the body remain unsubdued, and are too strong to admit of blood being drawn through the vessels to the periphery when heat is applied there. But if, while these spasms are being overcome by the application of ice along the spine, heat be simultaneously and energetically applied to the surface of the body, its effects will be wonderfully beneficial, because now the relaxed blood-vessels will permit the blood to pass, and the impulse

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\* "Eine bei weitem günstigere Beurtheilung als Blutentziehungen, so wie überhaupt die bei weitem günstigste unter allen dem antiphlogistischen Heilapparate angehörigen Mitteln, hat bei der Behandlung von Meningitis epidemica die äussere, örtliche Anwendung der Kälte, namentlich in Form einer auf Kopf und Nacken applicirten Eisblase, gefunden. Fast alle Beobachter, welche dies Mittel in Gebrauch gezogen haben, erklären sich mit der schmerzlindernden, beruhigenden und gleichzeitig belebenden Wirkung, die dasselbe auf die sensorielle Thätigkeit des Kranken äussert, in hohem Grade zufrieden gestellt, und wenn auch immer dahin gestellt bleibt, wie viel dasselbe zu einem glücklichen Ausgange des Leidens beizutragen vermag, so wird man, in Anbetracht der vollkommenen Unschädlichkeit dieses Mittels bei rationellem Gebrauche, um so mehr eine Indication für Anwendung desselben in dem von vielen Kranken lebhaft ausgesprochenen Wunsche nach der fortgesetzten Application der Kälte finden, die man aus diesem oder jenem Grunde auszusetzen für gut befunden hatte. Von einzelnen Seiten hat man vorgeschlagen, Eisblasen auch längs der Wirbelsäule zu legen; mit Recht erklärt Wunderlich dies Verfahren für ein mehr lästiges, als nützliches." *Die Meningitis cerebro-spinalis epidemica vom historisch-geographischen und pathologisch-therapeutischen Standpunkte bearbeitet von Dr. August Hirsch.* Berlin: 1866.

[The statement of facts in this extract is an extremely important and authoritative testimony of the remedial efficacy of ice in cases of cerebro-spinal meningitis: the assertion contained in the last sentence simply proves that neither Wunderlich nor Hirsch has yet attained a true conception of the *modus operandi* of ice when applied along the spine.]

given by the heat to the vital processes in the organic textures causes them again to attract blood to themselves, and thus to revive the circulation.

Moreover these local applications of heat are more effective than might, *à priori*, be supposed, even from what I have just said, for they exert a beneficial influence on the nervous centres. In slight cases, where the vasic spasms of the extremities are distinct, but not strong, vigorous friction or heat, so applied to one hand or foot as to make it thoroughly warm, will generally so act, as already explained, on the nervous centres related to the limb as to cause them, by reflex action, to relax the arteries of, and thus to ensure warmth in, the corresponding limb. And deductively, I conclude that this law obtains in every segment of the body.

The importance of making the utmost effort to facilitate the operation of the spinal ice-bag in relaxing the arterial spasms throughout the surface of the body is attested by a fact mentioned to me by Dr. Brinton,—viz., that if equal doses of opium be given to two dogs of equal size, and then if one dog be kept in a room at a low temperature, say 50°, and the other in a room of a higher temperature, say 70°, the former will die, but the latter will live. The cause of this difference in the fate of the two animals lies, I doubt not, in the fact that the constringing action of the opium on the peripheral arteries was assisted by the external cold in the case of the dog placed in the cold room; whereas the same action was impeded by the external warmth in the case of the dog placed in the warm room, and thus the circulation was kept up sufficiently to sustain life until the poison had been excreted or its force expended.

Now, if my hypothesis as to the nature of diarrhoea and cholera be correct, it is obvious that, as in cholera there is coldness of the surface (spasm of the peripheral arteries) induced in a manner analogous to that by large doses of opium, the experiment with the two dogs affords a strong proof that in the treatment of cholera the application of external warmth is likely to *aid*, to a great extent, in restoring the patient. As a matter of fact, I have succeeded, by the vigorous and persistent application of dry heat to the extremities of patients in collapse, in restoring a considerable amount of circulation in them; and Dr. Andrew Clark has informed me that by causing the arm of a collapsed patient to be immersed in hot water he has restored the pulse, previously extinct, in the radial artery. But, indeed, a very large amount of evidence exists that heat applied to the surface of the body exerts a considerable restorative power, which, though when used in cases of profound collapse only encourages hopes—to be soon cruelly disappointed, in cases of slight collapse is often, I believe, the means of arresting the further progress of the disease and of saving life.

When the principles on which heat is here recommended to be applied to the general surface of the body *simultaneously* with ice along the spine, are clearly understood, physicians can, of course, practise those principles in many different ways and yet attain the

same end. Of all the methods of applying heat, the most inconvenient and ineffectual is that by means of hot water in stone or glass bottles, unless the sides of the bottles be concave, in order thus to adapt them as far as may be to the convexities of the body. The ordinary round bottles can, of course, come in contact with a very small part of the surface of the body; they are very cumbersome, and uncomfortable to the patient, often impede his movements in bed, and thus subject him to an irritating restraint, or, if he is too ill to heed them, his first change of position is apt to move them from him. Bags made of calico or flannel, and containing hot sand or hot salt, are far more manageable and effectual. To be well adapted for their purpose, they should be flat and oblong, and of various sizes, adapted to the various requirements of the case. Care must be taken that the heat of the salt or sand, while considerable, is not great enough to irritate the patient, and that when the bags are filled, they should be so moderately filled only as to allow them to be thoroughly flexible, and therefore adaptable to the shape of the body. I think that it is much the best to make these bags of flannel, because as it conducts the heat more slowly than linen or calico it counteracts in some degree the almost inevitable discomfort of irritation occasioned at the moment when the bags are applied, however much care may be taken to make the salt or sand of suitable temperature. There are, however, in my opinion, two grave objections to these bags: first, the right preparation of them each time they are required for use involves considerable labour; and second, the difficulty of insuring that the salt or sand shall be of suitable temperature is almost insuperable, especially when it is necessary to entrust their preparation to uneducated assistants, who cannot be made to feel the necessity of complying with the requirement here insisted on.

The most convenient plan of applying heat which I have practised is that by means of india-rubber bags, containing water of suitable temperature. Last year at Southampton I used any spinal ice-bags or spinal water-bags which I might have at hand. Ice-bags of the largest size, filled with water, answer extremely well to place along the legs, and lesser ones or the water-bags are also very useful to apply to the arms, hands, feet, and abdomen. They are, however, of course not only too costly to be used systematically, but are much less suitable than other bags which might be made for the purpose. The india-rubber bags, used as ordinary foot-warmers, would answer well; but I should be disposed to recommend bags to be made of greater proportionate length, and of thorough flexibility, having, of course, the metal screw mouth-piece surrounded by an india-rubber guard to prevent it from irritating the skin. Such bags filled, not too full, of water, could be made to surround the limbs, by being tied on, and if diligently applied to all the coldest parts, would aid the spinal ice-bag immensely in producing reaction.

Another plan of applying heat—one which was used a good deal at Paris, in the Hôtel Dieu, during the cholera epidemic last



autumn, was a form of the lamp-bath, so arranged that the hot air from it is introduced into the bed of the patient. This plan is well known in England, and a lamp-bath with a tube adapted to convey the hot air from the lamp to the bed is manufactured and sold by Messrs. Benham, of Wigmore-street. I have had no experience of the efficacy of this plan of applying heat in cases of cholera, though I have often resorted to it in other cases. Although as a mode of treating cholera without the spinal ice-bag, its use does not seem to have been attended with more success than most other methods, when used co-operatively with ice along the spine it seems to me probable that it will be found peculiarly efficacious. Another and greatly improved form of applying heat on the same principle, consists in the use of moveable chambers containing hot air, and so constructed that either the whole or a part of the body may be exposed to the heat. For example, a large slipper-bath is constructed with double walls, between which the hot air is admitted. The inside of the bath is lined with wicker-work, in order to guard the patient's body from the hot metal; the patient being placed within the bath, his head of course being outside, and the bath being securely closed, the hot air between the two walls of the bath quickly raises the temperature of the air in the bath itself to the required degree. I have had no experience in the use of this bath, but believe it would be at once the most effective and practical, because portable, means for hospital use, of applying heat in cholera cases. These baths are, I believe, patented, and rather expensive. From the reports given me by two patients of the effects of the electric-baths used by Dr. Caplin, of Baker Street, I am disposed to believe they may prove very valuable. Mr. Urquhart has described a case in which cholera was cured by means of the Turkish bath: it is not unlikely that in slight cases heat thus applied will greatly facilitate recovery; but on theoretical grounds I am constrained to disbelieve that the Turkish bath, or any mode whatever of applying heat to the surface of the body, and even to the pulmonary mucous membrane also, will avail to recover patients from complete choleraic collapse. And I find it stated by Dr. Aitkin\* that "Turkish baths in the hospitals at Scutari have been used, but without benefit, and to the disappointment of the hopes which had been entertained of them." Theoretical considerations, however, lead me to think that if patients in collapse were placed in a Turkish bath while ice was being applied along the spine, the outside of the spine-bag being covered by an effective non-conductor, their chances of recovery would be the greatest; because hot air would then not only come in contact with the surface of the body, but also with the bronchial mucous membrane and lung tissue, and thus the same influence would be exerted in facilitating the re-flow of blood though the pulmonary vessels, the spasms of which were being simultaneously overcome by the ice along the spine, as would

\* "Science and Practice of Medicine," vol. i. p. 661.

be at the same time exerted by the application of heat over the surface of the body.

Experience only can positively determine which of these various methods of applying heat is the best, and I earnestly hope that physicians in charge of cholera wards in hospitals, where every facility for experimentation is obtainable, will give a systematic trial to each of these methods, in order to ascertain indubitably which of them co-operates most effectually with the spinal ice-bag in overcoming the vasic spasms.

Having shown the amount of efficacy of cold applied along the spine, and of heat applied over the general surface of the body, when they are used separately, and the indispensable necessity of using them conjointly in order to derive from them their maximum of remedial force, I now proceed to give such directions as seem to me necessary in order to insure the most effective use of both the one and the other. I must observe, however, most emphatically, in passing, that any one who uses the spinal ice-bag alone, in treating cases of choleraic collapse complies with only half the requisitions dictated by the principles I have laid down, and, therefore, is not treating the disease according to the method here explained.

I.—As soon as the symptoms of cholera show themselves, apply the spinal ice-bag along the central line of the back, letting the bag extend from the upper cervical to the third or fourth lumbar vertebra, or from the nape of the neck to the lower part of the hollow or "small" of the back. The bag may be applied either next to the skin, or over a linen or thin calico shirt or chemise. *It is much the best next to the skin*, and in no case must more than one layer of linen or thin calico intervene. At pages 31 to 34 inclusive, full directions for selecting a suitable bag in each case, and for preparing and applying it are given. These directions I earnestly commend to the attention of the reader. **THE REMEDIAL EFFICACY OF THE ICE IS GREATLY DEPENDENT ON THE CARE AND CORRECTNESS WITH WHICH IT IS APPLIED. IT MUST BE KEPT EXACTLY ALONG THE CENTRE OF THE SPINE: IF IT IS NOT, IT WILL DO HARM, AND HAD BETTER NOT BE USED AT ALL.** The only method of keeping it in its place, which does not involve incessant watching on the part of the nurse, is that of employing the "Ice-bag jacket" described at p. 33, and even then in cases of extreme restlessness, occasional slight re-adjustment may be necessary. In whatever way the bag be sustained in its place, it should be firmly pressed to the back, especially in the dorso-lumbar region, otherwise the full effects of the cold will not be experienced. This may be partially effected when the patient is lying on his side by placing a pillow or roll of sheeting so as to sustain and press against the bag; but by far the most effectual way is to cause him to lie upon it. Great care must also be taken that the cold does not come in contact with the base or back of the head, and that it does not reach downwards in any degree over the sacrum.

II.—Be very careful to retain whatever heat is still being evolved in the patient's body, by means of an ample supply of blankets, which should so effectually surround him, as he lies in bed, that the extremities, as well as the trunk of the body, are constantly well covered. In this respect, unremitting effort must be made to counteract his throwing off the clothes by his too frequently restless tossings. The ice-bag jacket, if made of flannel, or at all events if the sleeves are made of flannel, will prove a valuable aid in retaining warmth in the arms.

III.—Apply heat unremittingly along the extremities, in order to co-operate with the spinal ice-bag in overcoming their special coldness (vasic spasms).

IV.—Apply heat over the abdomen and hips for the same purpose.

V.—Take care to refill the spinal ice-bag with ice as soon as the previous supply is melted, and to keep the bag applied constantly along the spine as long as the cramps, purging, vomiting, and coldness of the surface of the body continue. Each bag of ice will last about an hour and a half, or from that to two hours. It is important to remove the bag before the water in it becomes warm, and immediately to refill it with ice. If the bag be allowed to remain on the back after it becomes warm, the india-rubber of which it is made tends to increase rather than diminish the heat, and, therefore, the hyperæmia of the spinal region.

According to my observations the cramps are the first of the group of symptoms to cease, and generally vomiting and purging are overcome a considerable time before reaction is established and the patient has become warm. A change, however, in the character of the skin, may often be observable within the first hour after the ice is first applied. Instead of remaining clammy it becomes dry, and simultaneously with the abatement of the cramps, vomiting, and purging, the patient gradually emerges from the algide state, which, of the four terrible symptoms of cholera, is the most formidable, and generally the last which wholly disappears. I believe that if all my injunctions for the treatment of cholera be duly complied with, the ice-bag may be advantageously applied until the patient becomes generally warm. But if heat be not applied to the surface, and if the patient be allowed to suck as much ice, or drink as much iced water as he wishes, there is danger that, after the spinal ice-bag has completely subdued the cramps, purging, and vomiting, it will, by unduly depressing the circulation in the spinal cord, exercise an injurious sedative influence on what I call the positive motor nerves distributed to the various structures of the body, and thus further progress in the direction of reaction will be arrested.

VI.—If, after the bag has been applied some time and the symptoms of the disease are subsiding, a special afflux of blood to the head, chest, or womb, should occur to an extent causing pain,—or, in the case of the lungs or womb, the threat of hæmorrhage,—care must be exercised so to regulate the force of the ice in relation to those particular nervous segments along the spinal region which

preside over the vascular system of the parts implicated, as to avert the consequences which might otherwise ensue.

When the preceding sentence was published as a part of the first edition of this book, I had had no experience of its immense importance in respect to cholera, but in writing it was guided by the admonition of theory conjoined with my numerous verifications of it in general practice. But as will be seen in the record of Cases VII. and VIII., treated by me at Southampton, 1865, and in Case XVI., treated there in 1866, that the warning which I had given is of paramount importance. Case VII., Mrs. B., ceased to menstruate a week before she was attacked with cholera; on the second day after the application of ice she began to menstruate again. Case VIII., Mrs. D., the patient being 73 years old, began to have hæmorrhage from the womb on the third day of treatment. Case XVI., Mrs. Peters, who concealed the fact of her pregnancy, aborted some days after treatment had been continued, which would have been considerably modified and more cautiously conducted had the fact of her pregnancy been known.

If one part of the body becomes warm before the other, the application of the ice must be modified accordingly. For example, if, while the lower half of the body still continues cold, and the vomiting or purging persists, the head and upper extremities have already become decidedly warm, the ice should be restricted to the lower half of the spine. Again, if simultaneously with the conditions just mentioned, there should be evidence of even incipient congestion, or the slightest hæmoptysis, then the ice should be restricted to the lumbar region only. Again, if before the algide symptoms are wholly subdued, there should be uterine pain or any evidence of an undue afflux of blood to the womb, the ice, if still necessary along the lower region of the spine in order to control the choleraic symptoms, must be applied with the utmost possible caution, its effects being constantly watched, and its use being modified according to the dictates of the changing symptoms. For example, while they continue severe it can still be applied, but as soon as they decidedly lessen it should be intermitted, and if they again strengthen it may be re-applied; the fact being always borne in mind that within half an hour, or even a few minutes, great changes in the circulation, and consequently in the functions of the organs related to the nervous centres acted upon, may be produced by the influence of the ice.

It may be stated as a general rule, the importance of which cannot be too strongly emphasized, that the sooner the ice is removed after it has subdued the symptoms against which it is directed the better; for as soon as it has done so its action, if still continued, will result in producing congestion of those very parts which it had just before rescued from spasmodic anæmia.

VII. Allay the extreme thirst during the stage of collapse by non-stimulating, or but slightly stimulating fluids, as hot as can be comfortably borne.

It is well to give the patient the choice of several beverage

indeed to allow him to vary them as he may feel inclined—for it is of the utmost importance to secure the co-operation of mental influences, which are no insignificant aids in allaying sickness. If beef-tea can be taken, it is, I think, the most desirable beverage. It should not only be hot and very carefully made, but should be temptingly flavoured. Much will depend on carefully ascertaining and complying with the taste and inclination of the patient. The drink which I rank next in importance to beef-tea, and which is on the whole more acceptable and retainable by the patient, is arrow-root made with water, flavoured, but only flavoured, with brandy, and given so thin that it may be readily drunk. Hot *weak* tea and coffee may also be freely resorted to; these had better be given, while any considerable sickness continues, without milk; a little brandy, say a teaspoonful, being added to each small cup. Hot lemonade will also be found a grateful and welcome beverage. This has the advantage of being the vehicle of a considerable amount of sugar. This nourishing substance should be freely added, having regard always to the taste of the patient, to any of the drinks he can take it in. Barley water may often be taken where arrow-root is disliked, and is also a very desirable beverage. If no fluid containing substantial nourishment can be taken, then I should recommend that very weak hot brandy-and-water be given in the proportions, say of about a tablespoonful of brandy to a tumbler full of water, sugar of course being added, if it can be taken in this case also.

I prescribe hot beverages during the stage of collapse as a means of raising the temperature of the blood, on the same principle as I apply heat to the skin. I do not ignore the fact that patients in collapse crave an abundance of cold, or iced water, and are glad to have pieces of ice to suck, and that this seemingly strong instinct should not be lightly disregarded. I am of opinion, however, that the first and greatest want of the patient is an abundance of fluid, and that the desire to have it cold, though partly due to the excessive activity of the gastric mucous membrane and glands, as well as to the large accumulation of blood in the spleen, is, in some measure at least, unconsciously prompted by long mental association of thirst with heat as its cause, and therefore by the habit of allaying extreme thirst by extreme cold. Physiological considerations seem indeed at first sight to supply a reason for giving to patients in collapse as much cold fluid as they will take: if the stomach be thus converted into a cold or iced-water bag, the cold is brought into more immediate contact than it can be otherwise with the great accumulation of nervous centres, called the semi-lunar ganglion or solar plexus. Appreciating this fact, I did not, when last at Southampton, insist so strongly as I did during my previous visit, that all the drinks given during collapse should be hot. Further consideration of the subject confirms my conviction, however, that cold fluids should be withheld. I am disposed to believe that when hot saline fluids are into the veins the temporarily beneficial results which are

obtained are mainly produced by the *heat* of the fluid injected, and thus imparted to the blood. Unless a careful statistical record of the results of treating a large number of patients by cold internally and an equal number by heat internally should ultimately prove that the foregoing reasons for withholding cold drinks and giving hot are not valid, I shall henceforth adhere to the practice during collapse of applying heat internally as well as externally at the same time. So far as the experience of one physician avails, the view here expressed is confirmed by the statement of Dr. O. H. Smith, of New York. He says that, "*Hot* lemonade and brandy, and *hot* coffee and brandy in small quantities, given by the stomach, will aid very much in hastening reaction, and in supporting the exhausted patient." He also says—"I observed that when brandy was thrown into the rectum, the next stool would be less in quantity. I then ordered brandy and strong coffee injection to one patient; brandy and a solution of sugar of lead to another; and brandy and a solution of tannin to another, and found they were all retained a considerable time, and each passage that followed was diminished in quantity. These injections were repeated after each passage, and each time the stools were considerably postponed and smaller, and in a little time the bowels were checked entirely. I then ordered brandy and strong tea, and afterwards brandy-and-water, by injection, with the same result, and found that brandy was the astringent and stimulant that controlled the watery stools. Patients would be brought in approaching collapse. They had passed much fluid by the bowels, and were continually having large watery stools every few minutes. They were in a cold and clammy sweat, the skin blue and shrivelled, the tongue cold, the voice husky, rolling and tossing in bed, and screaming with cramps in the legs. The nurses would take them by force and give them injections of two or three ounces of brandy, as much strong coffee or tea, and repeat the injection immediately after each passage; and usually three or four injections would control the passages, reaction would come on, the pulse would increase in volume, the surface of the body would become warm, the skin assume a more natural hue, in short, all the vital functions improve, and the patient go on to convalescence."\*

The immediate action of brandy and other alcoholic fluids on the mucous membrane of the alimentary canal is that of an astringent; but their ultimate action on the nervous system is believed to be such as, if the pathology of diarrhoea and cholera propounded be true, will be likely to prolong and intensify the malady, provided they be absorbed in quantities sufficient to affect the nervous centres in any considerable degree. During collapse, however, the absorbing power of the gastric and intestinal mucous membrane is so slight that this danger is not likely to be incurred until the patient has made some progress towards recovery from

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\* *New York Medical Journal*, Dec. 1865. The Epidemic Cholera of 1849 and 54, by O. H. Smith, M.D.

this state. Therefore, Dr. Smith's reported results harmonize with the views above expressed, and it is very desirable that his practice of constringing the intestinal glands should be repeated in order to ascertain whether an extensive experience will confirm his statement, and justify his treatment.

VIII.—Strychnia, opium, and all drugs which act as nervine stimulants, and which have any chance of being absorbed, should be scrupulously avoided. I am inclined to extend this remark to coffee and tea, if taken in strong solutions and when the patient is in such a state that any considerable proportion of them is likely to be absorbed.

IX.—Presuming as I do that the discharges are not eliminating any poison from the system, but are simply draining away the energy and life of the patient, I conceive it of paramount importance not only to arrest those discharges, but to compensate for the drain they have effected, or may be still effecting, by supplying the patient with a suitable nourishment as promptly and as abundantly as possible. Therefore, I advise that if the vomiting is allayed first, nourishment—beef-tea especially—be at once and repeatedly given by the stomach. If only a small portion of each supply be retained, that retention is already a great gain. Should, however, the purging subside before the vomiting, then frequent injections of hot beef-tea or arrow-root, or a mixture of both, or any other nourishing compound, should be persisted in. I cannot insist too strongly on the importance of this advice, for I am quite satisfied that even though all the directions previously given be complied with, if this one be neglected, many sufferers who might otherwise be saved will be lost.

X.—The foregoing directions wholly relate to the treatment of the incipient and algide stages of cholera. A large number of patients whose lives are prolonged through these stages sink during the period of reaction, the fever, feverishness, or local congestions of which need the most studious attention, in order that they may be prevented or remedied by the judicious use of heat to the appropriate part of the spine. I have already suggested that in cases where cerebral or pulmonary reaction has become established while vomiting, purging, or coldness of the lower parts of the body persists, the ice should be omitted from the upper segments of the spine. But cases occur in which purging, at all events, as well as coldness of the lower extremities, co-exists with a full reaction, merging into slight or severe congestion of the brain, and sometimes, though less frequently, of the lungs. In these cases it may not suffice merely to omit the ice from the upper half of the spine, but it may be necessary, while still controlling the purging by retaining the ice in the lumbar and lower dorsal regions, and while aiding its action in relaxing the vasic spasms of the lower extremities by heat directly and effectually applied to them, to effect some contraction of the cerebral and perhaps pulmonary blood-vessels by the application of heat along the upper third, or upper half of the spine. To do this effectually,



and at the same time not to over do it, calls for the utmost discrimination and care,—discrimination with reference to the controlling force needed, and care with respect to the temperature of the water used, and the length of time during which it is applied. The changes in the circulation which may be induced by the spinal water-bag are much more rapid than those inducible by the spinal ice-bag, therefore the action of the former must be watched even more carefully than that of the latter. If heat be applied in order to prevent or subdue cerebral congestion, the forehead and pupils of the patient must be scrupulously examined every few minutes in order to ascertain whether the temperature of the former is falling, and whether the size of the latter is increasing: if so, the bag should be immediately removed, even though it may be desirable to apply it again a short time afterwards. The condition of the lungs may be ascertained not only by the feelings of the patient, by the sputa, and by the degree of ease with which he breathes, but of course much more surely by auscultation; and the use of heat must be determined by the symptoms. If there should be no special but only a general feverishness, or even a decided reactionary fever, this condition may be controlled by the application of heat along the whole spine. It may be stated as a general rule, that in proportion to the gentleness and slow graduation with which the fever, feverishness, or local congestion is controlled, the more satisfactory will be the result, and the less the chance of relapse. If, for example, in order to diminish cerebral hyperæmia, water of a high temperature in the spinal water-bag be applied along the upper third of the spine, the head will, in many cases, become rapidly and extremely, and it may be, in some cases of cholera, fatally cold. After such rapid and extreme contraction of the cerebral arteries, and the necessarily sudden removal of the water-bag, there is danger of vigorous reaction. I advise that the temperature of the water used in these cases be of the lowest consistent with slowly attaining the desired end. The vascular system of many patients may be influenced by water at 105° Fahr. It will, however, be found most available at 110°, and may range, particularly if the patient's body be well clothed with fat, to 115°, or even higher.

It must be well understood that the application of heat along the spine is capable of producing vomiting and purging after they have been thoroughly subdued by means of ice, and of causing the body again to become cold. It is impossible, therefore, to overstate the importance of the advice to use these agents only when they are absolutely needed, and to watch their effects with the utmost possible care. The intensity of the cold or heat which is applied, and the length of time it is used, being judiciously determined and modified according to the exigencies of each case. Perhaps the greatest objection which can be alleged against these remedies is their potency, and therefore, inasmuch as they act with great rapidity, the need of constant watchfulness in order to avert the

disastrous consequence of their excessive action. It may, with peculiar appositeness be said of them what the proverb says of fire and water—"they are good servants, but bad masters."

XI.—The chief things required by patients who have fairly passed through the algide and reactionary stages of cholera are good nursing and good nourishment. No medicine is absolutely necessary, unless to meet some special symptom; but I am of opinion that a gentle tonic is desirable, and that that tonic should consist of the citrate of iron and quinine. I cannot conclude this section more appropriately than in the following words of Sir Ranald Martin, for it is impossible to over estimate their importance:—"In cholera, in common with the last stages of violent fevers and dysenteries—as, in fact, in all diseases of great exhaustion—the patient will always owe much to the horizontal position, and to careful and unremitting nursing. The most careful nursing and the most attentive watching of the patient are both of the utmost importance in this disease; and so easily is the balance of circulation fatally overturned, that a strict attention to the recumbent posture is absolutely necessary to success. In no other diseases are these simple matters of so great importance to be attended to, and in the disease under special notice I have seen many a life apparently lost from inattention to them."

## SECTION II.

### *Treatment by Medicines.*

IN attempting to form any estimate of the comparative value of different medicines used in the treatment of cholera, the following facts must always be borne in mind. The mortality of the disease differs—*first*, in different years; *second*, in different seasons of the year; and *third*, during the different periods—the outset, development, and decline—of each epidemic. This last fact is well illustrated during the outbreak at Kurrachee, in Scinde. Of the

First 100 patients admitted into hospital 79 died.

Second	"	"	66	"
Third	"	"	50	"
Fourth	"	"	40	"

The treatment all the time remained essentially the same. Moreover, the effects of medicines differ widely according to the stage of the disease in which they are given: during the premonitory stage and during reaction they are powerful for both good and evil; but during partial collapse they are absorbed only slightly, and during profound collapse scarcely at all. These considerations, as well as many others which I need not recapitulate, must be taken into account before an *accurate* appreciation of the statistical results of different methods of treatment can, if it ever can, be arrived at.

The following returns of the Medical Council of the College of Physicians must therefore be accepted as an *approximative* estimate. It is, however, the best available, and probably sufficiently near the truth for practical purposes. Considering the different methods of treatment when applied in the various stages—from choleraic diarrhœa to profound collapse—the *general* per centage of deaths following each plan was as follows:—

Eliminants . . . . .	71·7 per cent.
Stimulants . . . . .	54 „
Alteratives—calomel and opium . .	36·2 „
Astringents—chalk and opium . .	20·3 „

But when the different methods of treatment were tested in their application to cases of collapse only, the results were as follows:—

Calomel and opium . . . . .	59·2 per cent.
Calomel (large doses) . . . . .	60·9 „
Salines . . . . .	62·9 „
Chalk and opium . . . . .	63·2 „
Calomel (small doses) . . . . .	73·9 „
Castor oil . . . . .	77·6 „
Sulphuric acid . . . . .	78·9 „

It is obvious that in a very considerable proportion of the cases from the treatment of which the first of the above statistical statements is generalized, the absorbent process was still very active; for otherwise the wide difference in the results of the different methods would not have been possible—differences ranging over 50 per cent. In the more advanced stages of the disease, or during collapse, the differences according to the second statement have a range of nearly 20 per cent., a fact which indicates that absorption in a slight degree still goes on.

I have already adverted to the *modus operandi* of evacuants, and have explained the reason of their dangerousness; and what I have said concerning alcohol in Chapter IV. renders it needless for me to advert here to the effects of stimulants. I shall therefore restrict the following remarks to calomel, opium, sulphuric acid and other astringents, the bromides of potassium and ammonium, quinine, arsenic, and nitrate of amyle.

*Calomel* has been, and probably still is, the medicine most extensively used in this country. Its use was advocated on theoretical grounds by Dr. Ayre, who maintained that the phenomena of cholera are *proximately* due to disorder of the liver and arrest of its functions; but, as observed by Dr. Gull, “the theory of the disease which has chiefly led to its employment is not supported by anatomical facts. The absence of bile appears to be merely a subordinate result.” This statement is confirmed by the doctrine I have put forward, that the function of the liver is arrested simply because the supply of blood is cut off from it by spasms of the hepatic artery and portal vein. And if so, it is obviously absurd to give calomel with the intention of restoring the hepatic functions. The organ



is in respect to its intimate structure in perfect condition, and only needs arterial blood to nourish it, and portal blood to act upon, in order to resume its functions at once. If when calomel is given the portal vein be still open, the drug, so far as the liver is concerned, though gaining access to the organ, is needless, for its functions are still performed healthily; and if when the drug is given the portal vein is already closed, it is useless and sometimes dangerous, for gaining no access to the liver it cannot act upon it, and hence is either ejected with the evacuations, which it is likely to increase, or remains unabsorbed in the alimentary canal, where in fatal cases it has often been found. In cases of recovery it probably exercises not unfrequently a baneful influence during the period of reaction, and occasionally it produces salivation. Whenever it acts beneficially, if it ever does so, and many seem to believe it does, for many still administer it, its influence is probably exerted directly on the nervous system. Considering its disintegrating effects on inflamed structures, and arguing analogically, it is not unreasonable to conclude that it may, when absorbed, exercise a devitalizing, and therefore weakening influence on the excessively energetic spinal cord and sympathetic ganglia. But assuming the operation of such an influence as the only rational justification of the use of calomel in cholera, it is clear that the benefit derived from it can be obtained with far more certainty from the spinal ice-bag. If any benefit is ever derived from calomel, the extent of that benefit is indeterminable and indeed very dubious; and it is always impossible to know whether, if it is administered, any of it will be absorbed, and if so to foretell its effects.

*Opium* has played, and still plays, an important part in the treatment of cholera; it is a powerful agent both for good and evil, and while, owing to its peculiar modes of action, it holds out strong temptations for the physician to prescribe it, its administration to cholera patients is beset with great and acknowledged danger. As I believe the reason of its peculiar dangerousness when given to cholera patients is not fully understood, I shall devote a few pages to the discussion of its influence, and the mode in which that influence is exerted. The chief or most notable action of opium consists in causing first hyperæmia and then congestion of the nervous system. How it does this—what abnormal chemical changes occur, what normal chemical changes are impeded or arrested—is not known; as yet our knowledge of the matter is limited to a partial insight into the physiological action of the drug, and this is exerted on the three great divisions of the nervous system in somewhat the same order as is that of chloroform: in man the cerebrum is first involved; then the sensory ganglia; then the automatic nervous centres, consisting of the spinal cord, and the ganglia of the sympathetic. Preluded or not as it may be—according to the patient's idiosyncrasy and the quantity of opium taken—by excitement, congestion of the cerebrum takes place and is denoted by mental heaviness, apathy, and inaction.

Then, as the effects of the drug deepen, perceptive power gradually declines; the lacklustre eye, if opened, tells how indubitably visual power is impaired, and it becomes necessary to shout to make the patient hear. Then total blindness and deafness follow: the closely contracted pupil denotes the complete congestion of the optic ganglia, and the loudest shout evokes no response. Then follow increasingly decisive signs—first of hyperæmia and afterwards of congestion of the ganglia of the sympathetic and of the spinal cord: the arteries of the skin become contracted, thus causing coldness and pallor of the surface; sweat is freely secreted; the cerebral arteries contract, thus causing cerebral anæmia to succeed to cerebral congestion; the forehead, as well as the face, at first flushed and hot, now becomes deadly pale and cold; the dilatation of the pupil which now occurs denotes not only that the cerebral congestion has ceased, but that the spinal cord has become hyperæmic; and the inner skin—the mucous membrane—as may be demonstrated in the case of that lining the bronchial tubes, exudes its appropriate secretion, as the outer skin does, in excessive abundance, in such abundance indeed that in some cases the bronchial tubes are filled with it, and the patient dies of suffocation.

The kidneys first secrete an excessive abundance of urine, and then in some cases cease their functions—results, as I have already stated, of spinal hyperæmia; and there is not unfrequently involuntary expulsion of urine and fæces. It is said that the exertion of the poisonous influence on the cardiac ganglia helps to impede the action of the heart, but the extreme slowness and weakness of the pulse in cases of opium-poisoning is adequately accounted for by the arterial spasms obtaining throughout the body: the volume and velocity of the blood-currents in all the peripheral arteries are lessened; the blood being to a great extent shut off from the capillaries, the chemical processes of textural nutrition and transformation are partially suspended, and hence that chemical blood-moving force which is exerted within the area of both the pulmonary and systemic capillaries, and to which the heart's actions are responsive, being in comparative abeyance, this negative state, in conjunction with the vasic spasms, necessarily induces a slow and increasingly feeble pulse; and as the function of respiration is indissolubly associated with that of circulation, it must, irrespective even of the state of the medulla oblongata, also become gradually slower and more laboured, and finally irregular and gasping, until at last it ceases altogether.

In man, the high degree of development of his nervous system, and especially the large size of his brain, must, to a considerable extent, mask the action of opium on the spinal cord; but in animals, in proportion as the spinal or automatic nervous system predominates, the paramount action of the drug on that system becomes manifest. In fishes and reptiles, convulsions are among the first notable symptoms which opium induces. In frogs, evidence of the most extreme action of the spinal cord—viz., *tetanic* convulsions—is the

result of the administration or application of opium. In eleven out of twelve experiments by Dr. Anstie on dogs, cats, and rats, large doses of morphia caused convulsions in one shape or another. "The violence of the convulsions, and the frequency of their recurrence, was proportionate to the largeness of the dose." Children resemble the lower animals more than adults do in respect to the great relative predominance of the spinal cord, and of those nervous centres which preside over the vascular system; and accordingly, in them, coma, with clonic convulsions, is induced by opium with great rapidity; and the causes of cholera operate with far more destructive effects in childhood than at any subsequent period of life. The lower races of men occupy positions between those of children and adults of the Caucasian race, and it appears that opium produces convulsions more frequently in the negro, the Malay, and the Javanese, than in the European.\* Convulsions induced by opium in European adults are, however, met with oftener perhaps than is generally supposed. Professor Christison has recorded several cases.†

There are good reasons for believing that when, in cases of disorder of the nervous system, the pupil is dilated, the chief seat of irritation and congestion is in the spinal cord and collateral ganglia of the sympathetic; and that when, in cases of narcotic poisoning, the pupil is dilated, those parts are implicated in a paramount degree, the cerebral blood-vessels being relatively less distended than those of the spinal cord and sympathetic. Now, in many animals poisoned by opium, dilatation of the pupil occurs at a much earlier stage of narcosis than it does in man; in the cat, a poisonous dose of opium "invariably causes wide dilatation and immobility of the pupil *at once*, while the other paralyzing effects on the nervous system are developed in a far more leisurely manner; for instance, the animal retains consciousness for a long time after the dilatation of the pupil—a state of things the very reverse of what happens in adult men, though *I have seen it occur more than once in young children.*"‡

The comparative earliness of manifestation of stupor in cases of opium-poisoning is, I believe, determined by the comparative ascendancy (activity as well as size), and, therefore, vigour of circulation in the cerebrum. The more active its functioning power, the more rapid and intense the chemical transformations of its vesicular matter, and, therefore, the fuller and stronger the blood-currents to and from it. Hence it is that as the paramount or most active nervous centre attracts blood (and in cases of opium-narcosis the poisoned blood) most copiously and speedily, it becomes poisoned first; and hence it is that the evidence of the poisonous influence, viz., stupor,

\* Pereira's "Materia Medica," Vol. II., part II. p. 2108.

† Vide "Christison on Poisons," p. 707 (Fourth Edition), cited by Dr. Anstie.

‡ "Stimulants and Narcotics, their Mutual Relations." By F. E. Anstie, M.D., p. 231.

appears earliest in the highest organism—the Caucasian adult, and, in vertebrate animals, relatively to the other symptoms, later in proportion to their lowness in the scale of development.

Birds, however, occupy an exceptional position: not only does the general circulation of the blood attain in them its maximum degree of rapidity, but, corresponding to the extraordinary amount of their intelligence relatively to the size of their brains, the cerebral circulation is peculiarly quick and vigorous. Hence, as might be expected, when birds are subject to the poisonous influence of opium, stupor is induced in them earlier, relatively to the other symptoms of narcosis, than it is in most of the mammalian tribes, although the latter rank above them in the order of general development. In respect to the law in question, birds are an example of the common saying, "Exceptions prove the rule," which is also fully borne out by the order of manifestation of the cerebral symptoms of opium-poisoning in man: in the adult, stupor is one of the earliest phenomena; in children, consciousness is retained relatively much longer—in some cases, as already stated, for a long time even after the pupil has become dilated.

In interesting accordance with the view of the facts just presented are certain others, the conjoint significance of which has, I believe, been only partially appreciated, and the right understanding of which will result in important therapeutical consequences:—*First*, patients suffering from copious hæmorrhage can take an extraordinary quantity of alcohol or opium, not only without experiencing the usual poisonous effects of either the one or the other on the brain, but with positive benefit. *Second*, patients suffering from delirium tremens can often take an extraordinary quantity of alcohol or opium, not only without experiencing the usual poisonous effects of either the one or the other on the brain, but with apparent benefit, including the induction of sleep. *Third*, other patients suffering from the same disease may take the same quantity of alcohol or opium, not only without producing sleep or any apparent benefit, but may seriously increase their malady. *Fourth*, other patients suffering from the same disease may take large and repeated doses of opium, and, while wholly failing to induce sleep, may "die from collapse, almost as suddenly as if they had been shot, or had swallowed a large dose of prussic acid."\* *Fifth*, in cases of tetanus, doses of opium large enough to kill healthy persons may be administered without producing apparently any narcotic symptoms

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\* "It has happened to me, as I doubt not it has to others, to see patients suffering from delirium tremens, who had been injudiciously pressed with large doses of opium, because smaller ones had failed to procure sleep, die from collapse, almost as suddenly as if they had been shot, or had swallowed a large dose of prussic acid, most obviously from the effects of the medicine. Dr. G. Johnson, who has kindly favoured me with his views on this subject, insists very strongly on this evil influence of large doses of opium in delirium tremens."—*Antic on Stimulants and Narcotics*, p. 170.



whatever. Now, if I read these facts rightly, their meaning is as follows:—

**FIRST CASE.**—When there has been great loss of blood, the brain, which relatively to its bulk receives the largest supply, feels the drain the soonest, and ceases its functions earlier, therefore, than the automatic nervous centres do; hence, with respect to its functioning power, it is temporarily degraded to a position, relative to those centres, analogous to that permanently occupied by the brains of the lower vertebrata, and hence convulsions are readily induced; therefore alcohol, by virtue of its special power of attracting blood to nervous tissue, exerts a beneficial influence by restoring blood to the brain, which, being previously bloodless, permits the continuance of that influence for a long time without the production of its usual consequence—cerebral congestion.

**SECOND CASE.**—The phase of cerebral congestion induced by alcohol having been passed through, and the sympathetic nerve being now under the influence of the poison, its hyperæmic state causes contraction of the cerebral arteries, and therefore cerebral anæmia. Fresh draughts of alcohol, or full doses of opium, restore the attractive force of the brain tissue for blood; and as the sympathetic nerve centres are not yet so completely under the toxic influence as to counteract that force by inducing irresistible contractions of the cerebral arteries, the blood flows in them in fuller streams than before, and thus the famished brain receives new nourishment, and assumes the state associated with the process of nourishment—the repose of sleep.

**THIRD CASE.**—The poisonous influence of the alcohol has now acquired such an ascendancy over the vaso-motor nerve centres that further supplies of alcohol, or full doses of opium, not only fail to re-establish that condition of the cerebral circulation constituting the ordinary initial stage of alcoholic effects, but the cerebral arteries are so closely clasped by their muscular coats, as to prevent even that moderate flow of blood through the brain which is essential to that process of nourishment forming a part of the function of sleep. The force of the poison being chiefly expended upon, or rather manifested by, the sympathetic, its action is disproportionately intensified, and the malady therefore is in the same degree increased.

**FOURTH CASE.**—It is obvious, that if the condition just described were heightened but in a slight degree, the brain would be deprived of blood to an extent incompatible with the continuance of its life: and as simultaneously the nourishment of all the other organs of the body is being shut off from them, though in lesser degrees than that of the brain, the conclusion is inevitable that such patients, “injudiciously pressed with large doses of opium, because smaller ones had failed to procure sleep,” must “die from collapse almost as suddenly as if they had been shot.”

**FIFTH CASE.**—Tetanus is a disease consisting primarily of

hyperæmia, and, in advanced stages, of inflammation,—proceeding even to disintegration of the spinal cord. Now, as I have already said, when a certain condition of defect or excess of blood has been induced in any given segment of the nervous system, a like condition, though less in degree, will be assumed by the proximate segments. Thus it is, that in cases of tetanus, the excessive hyperæmia, of the spinal cord is partaken of by the collateral ganglia of the sympathetic, and hence, associated with the tetanus of the voluntary muscles, is a tetanoid condition of the involuntary muscles, and most especially of those governed by those ganglionic centres ranged most closely along the spine. Now the upper part of that chain of ganglia governs the cerebral arteries, which therefore are spasmodically contracted, and held so firmly in the grip of their surrounding muscles, that even under the strong stimulus of large doses of opium the cell-tissue of the brain is powerless to attract blood through them in currents of sufficient volume to produce cerebral hyperæmia—the first stage of opium poisoning, while the intense irritation arising from the diseased state of the cord is propagated to the brain to such an extent as to prevent sleep. Of course, though in these cases “doses of opium large enough to kill healthy persons may be administered without producing apparently any narcotic symptoms whatever,” the drug is not inoperative: its influence on the brain is counterbalanced, and so far the exertion of its narcotic power is masked; but its influence on the sympathetic and on the spinal cord is in no degree opposed. On them it expends its full force, and, hidden from physical vision by co-operating with and intensifying the disease, hastens the death of the patient.

Clinical records abound with evidence which might be cited in support of the doctrine here announced, viz., that in cases of undoubted spinal hyperæmia, or congestion, the administration of opium does but increase the symptoms and the danger. Three such cases published in 1865, are so remarkable and instructive, that I am tempted to give an abstract of the reports of them here:—

CASE A.—A young woman, eight months pregnant, who experienced a shock from the unkindness of her friends, complained on the 26th of the month of constant twitchings of the muscles of the arms and legs, with an evident inability to control their movements, sleeplessness, rather severe frontal pain, considerable heat of head, quick pulse, slight thirst, and some difficulty of speech and deglutition. On that day she was ordered “six leeches to the temples, to be followed by cold lotions to the head, purgatives, and milk diet. Next day, the 27th, she was evidently worse, excepting that the leeches had relieved the pain in the head.” Then she was ordered “a steel mixture and two opiate draughts, each containing half a drachm of *tinctura opii*, one to be taken at bedtime, and repeated if necessary. 28th.—The jactitation of the limbs is increased in severity, so that four strong women can scarcely keep her in bed.” In the hope of

mitigating the nervous symptoms, the induction of premature labour was decided upon (would not this have increased the spinal irritation?), and, as a preliminary, chloroform was administered (further irritation of the nervous centres). Labour was found to have begun without artificial aid, and was completed. "After delivery no diminution took place in the jactitatory motions, and," says her medical attendant, "I prescribed *two grains of acetate of morphia*, to be given in divided doses until sleep was induced. 29th.—Has had no sleep, though she took the whole of the morphia. Her condition remains unchanged. Prescribed *full doses of chlorodyne*, with the hope of producing sleep, but it took no effect, and she gradually sank exhausted, and died at two o'clock on the morning of the 30th."

The post-mortem examination revealed "the membranes of the brain rather congested," and evidence of acute, destructive inflammation of the spinal cord.

Now I beg to submit for the consideration of the profession the question—Was not this a case in which mere hyperæmia, not exceeding the limits of transient congestion of the spinal cord, was developed into fatal myelitis by the free administration of opium, morphia, and "full doses of chlorodyne"?

CASE B.—A widow, aged 33, who "had no appearance of being seriously ill," was admitted into a hospital on the 12th of the month. "She stated that she had suffered for six months from indigestion, brought on by worry and over-exertion. The last eight days she had been very sick, vomiting up all her food. . . . Tongue coated; is very thirsty; bowels relaxed lately; urine dark coloured; pulse very weak and small. She feels very weak; is almost ready to fall when she stands up; has much flatulence." She was ordered *brandy* and milk at short intervals, *ice to suck*, and *beef-tea*. On the 13th, as the sickness was not better, she had *five grains of calomel* and a *sinapism* to the epigastrium. The next day there had been no action of the bowels, and *Indian hemp* was ordered, *gr. ½ quater die*. 15th.—Is less sick. 17th.—Not much sickness; bowels not open; skin cold; and pulse extremely small and weak. . . . Feet rather cold; is lying lightly covered; complaining of having been too hot; face looks quite rosy and well. *Brandy 5oz.* . . . 19th.—Was yesterday apparently moribund and almost pulseless; eyes venously injected; hands cold; but quite conscious. Had vomited a large amount of dark liquid stuff. Ordered *opii gr. ½, 2 dis horis*. To-day, contrary to all expectation, she is still alive, quite conscious and rational, but in the same state of utter collapse and pulselessness: hands livid and cold; their temperature 71°b; that of axilla, 92°. . . She was now packed in a hot wet sheet, but no good effect was produced; the fingers turned blue, and the circulation gradually ceased. Death occurred at 8 P.M. *Brandy* had been given freely during the last twenty-four hours, besides claret."

The post-mortem examination revealed nothing which could be regarded as an adequate cause of death. The physician who attended her observed, "It seems impossible to account for the gradual failure of the circulation on the view that the strength was exhausted by the vomiting, for this was certainly by no means so frequent and distressing as it is in many cases which are in no peril. . . . The duration of the malady was far too long to admit of its being ranked as a case of ordinary choleraic affection: nevertheless it seems to me most probable that the collapse was produced by some toxic influence acting on the nerves of the heart and arteries similar to that of the epidemic disease."

Holding the views I do concerning the nature of cholera, and the *modus operandi* of alcohol and opium on the automatic nervous centres, it seems to me probable that the "toxic influence" to which the collapse is ascribed was exerted by the free administration of brandy and opium to a patient whose spinal cord and sympathetic nervous system had already become hyperæmic, and therefore peculiarly susceptible to their action.

CASE C.—A man, aged 60, admitted into hospital on the 13th of the month, and attended by the physician who had the care of the patient just mentioned, "was taken ill on the 9th with pain in his bowels, which has continued ever since. At present he is very prostrate, with cold skin, thready, scarcely perceptible pulse, coated tongue, hiccough, frequent vomiting, which he has had four days, while the bowels have been confined during the same time. The abdomen is distended, is dull in the depending part, resonant in the upper; the pain in it has diminished to-day. *Opii gr. ½ o. horâ ad vices iij. Brandy every quarter of an hour.* 14th.—Was very sick part of previous evening; remains in same state as yesterday, except that diarrhœa has come on and is almost constant since 10 A.M.; it began at 6 A.M. Stools fluid, bilious, and offensive. Sickness has been checked by *repeated doses of chloric ether*. Pulse barely perceptible; tongue extremely coated; body exhales a cadaverous odour; is quite conscious, but can scarcely speak; died soon after.

"At the post-mortem examination nothing whatever was found to explain the symptoms."

It is to be regretted that the amount of brandy given in the two last cases, is not, except on one occasion, stated. The physician observed, "The resemblance of this case to choleraic disorder is sufficiently apparent, and as it occurred at the same time as the one first related, there is some additional probability that both were of the same nature," *i.e.*, due to the same "toxic influence." Whatever that "toxic influence" may have been, it is quite clear that neither brandy nor opium availed in the least to arrest the disease, and that from the hour they were given death advanced in this case as well as in the other, with increasingly rapid strides.

In the chapter on the Causes of Cholera I have briefly explained why it is dangerous to give opium to cholera patients in collapse; but I hope the foregoing exposition of the dynamic influence of opium will render my meaning wholly unmistakeable. If the views just expressed be correct, it follows that no physician is justified in concluding that his patient "tolerates" opium safely, because he retains his consciousness and evinces none of the usual narcotic effects of the drug; for precisely in these cases is there the greatest danger. The spinal cord and sympathetic ganglia of a patient in collapse are, according to my hypothesis, in a state of extreme hyperæmia, and hence, if opium be given that condition will be intensified. If, however, before taking the opium, the cervical sympathetic ganglia are not already so intensely hyperæmic as to effect the contraction of the cerebral arteries to an extent sufficient to resist the fresh attractive force set up by opium between the brain-tissue and the arterial blood, a dose of opium will increase the cerebral circulation, and will probably induce congestive sleep. Such a sleep will prove that the state of collapse is not very profound, and that the chances of recovery are therefore considerable; while the sleep obtained, by increasing the strength of the brain relatively to that of the automatic nervous centres, will probably make those chances stronger. Meanwhile, the opium coming in direct contact with the glandular surface of the alimentary canal will obtund the nervous fibrillæ distributed to it, and thus, temporarily at least, will impede, if it does not arrest, secretion. In this manner, opium may in certain cases even of collapse, when not profound, prove decidedly beneficial. Hence it is, that "if," as Mr. Orton\* observes, "drowsiness were produced by the narcotics, after sleep the patient generally awoke much relieved, with a warm equable sweat all over his body." Unfortunately, however, it is impossible to be certain what cases of collapse are precisely in that stage when opium will act in the manner just described.

On the other hand, if the hyperæmia of the sympathetic be already such as to constrict the cerebral arteries so energetically as to withstand the heightened attraction, induced by the opium administered, of the brain-tissue for arterial blood, and thus to prevent any fresh afflux of blood to the brain, the great advantage of the opium—sleep, will not have been obtained; while its direct influence in impeding secretion will be but temporary, and its full force unrestrained by the increasingly enfeebled brain will be expended on the automatic nervous centres, which, being thus stimulated, intensify all the symptoms of the disease, and, therefore, its tendency to a fatal issue. "Such is the tolerance of opium," observes Dr. Macpherson, "that no such effect [sleep] is often produced by even large doses of the drug." In such cases, as I maintain, its danger is the greatest. There has been considerable apprehension that its

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\* Quoted by Dr. Macpherson, p. 112.

dangerousness is chiefly manifested during the state of reaction, which it is supposed to intensify: there is probably some ground for this, inasmuch as if it intensifies the spasmodic condition of the vascular system during collapse, it will thus cause the reaction to be correspondingly increased. The danger from this mode of its action is, however, not likely to be great: its really baneful influence is exercised, as above explained, in cases of collapse, and is most energetic in the worst cases, and when, the kidneys having ceased to act, the poison is not excreted from the system.

The beneficial influence of opium consists in its power of rapidly increasing the amount of blood in the brain, which is always below par in all the stages of cholera until reaction sets in, and in impeding or arresting secretion throughout the alimentary canal. When a person receives a mental shock, by the reception of bad news, for example, the cerebral circulation is suddenly lowered, and the automatic centres as suddenly become hyperæmic. The latter change is often evinced by an immediate action of the bowels, and the discharge of a large quantity of pale urine. It seems as if, when the normal amount of blood in the brain is lessened, and its regulative predominance over the spinal cord and sympathetic ganglia is correspondingly impaired, they immediately become unduly active. Now, if a person who has received a shock, as just supposed, immediately drinks a few glasses of wine, the full circulation of blood in the brain is artificially restored, the mind recovers itself, and, meanwhile, the automatic nervous centres resume their normal functional activity. As already said, during diarrhœa and the onset of cholera the brain circulation is lowered, and when, in these cases, opium is given, its main action is, I believe, like to that of wine in the case of shock, as just explained.

But opium exerts a specific restraining influence over diarrhœa by coming in direct contact with the glands of the alimentary canal: it probably induces extreme congestion of the nerves presiding over the glands, and that deadening them temporarily stops their action, and, therefore, that of the glands over which they preside. In this manner secretion is *temporarily* restrained while the cerebral circulation is increased, and when these advantages are not paid for by the too high price of producing insuperably persistent hyperæmia of the spinal cord and sympathetic ganglia, with subsequent increase of secretion—a risk always run—the patient is cured. If I had a spinal ice-bag at hand I should not fear the risk, but if not, I should hesitate to have recourse to opium.

*Sulphuric Acid.*—There is a general concurrence of opinion in favour of sulphuric acid as one of the best antidotes of diarrhœa; but though it has been enthusiastically praised for its great curative powers in cases of cholera, and though the other mineral acids have also their advocates, who affirm their efficacy in the treatment of the disease, I believe on *à priori* grounds that the mineral acids generally are among the most dangerous medicines which can be given to cholera patients. Given in medicinal doses, sulphuric acid

acts chiefly as an astringent, and as it comes in direct contact with the glandular system of the alimentary canal, its astringent action is first and immediately excited where it is most needed, viz. on that system the blood-vessels of which become contracted. Meanwhile the result is diminution of secretion. The acid does not affect the nervous centres injuriously: it does not increase their hyperæmic state in any degree: but, on the contrary, if it exerts any influence upon them it is of an opposite kind. Thus the mechanical action of the acid on organic tissue accords with experience of its therapeutical results: it stops diarrhoea, and it exerts no evil influence simultaneously on the nervous system. But when cholera is already so far developed that the arteries generally have become considerably contracted—a change denoted by a decided fall in the bodily temperature, sulphuric acid is, in my opinion, a very dangerous medicine. If its action could be restricted to the alimentary canal, that action would, I believe, be unmixedly good; but to the extent that the medicine enters the arteries evil ensues. It constricts them, especially their smaller branches, as well as the capillaries; and as, by excessive nervous energy, they are already so contracted as to produce the algide symptoms, the superaddition of the constringent power of the sulphuric acid has, I venture to affirm, often turned the scale in favour of death. It will be seen that this view of its action explains, and is confirmed by, the very large mortality attending its use—78·9 per cent., a higher rate than that following any other method of treatment the effects of which were reported by the Medical Council of the College of Physicians. As already mentioned, I have found that sulphuric acid, when given even in very small doses to peculiarly sensitive persons, merely as a tonic, will in some cases produce serious embarrassment of the pulmonary circulation, with a dragging sensation in the chest; and, I may add, that I feel grave compunctions for having once given seven minims of the dilute acid in a case of choleraic collapse; for, apparently, such were the suffocative effects of the drug, that I fear it hastened, at all events, the death of the patient. I am glad to find that my opinions concerning the action of sulphuric acid are completely sanctioned by the recent experience of Dr. Andrew Clark, who, when I mentioned to him my view of the *modus operandi* of the drug, informed me that he had found it extremely beneficial in the treatment of diarrhoea, but quite the contrary in the treatment of cholera, and that my explanation of why the results of its use in the two stages of the disease are so widely different, seemed to him the true one.

Reviewing the action of opium and of sulphuric acid, we see that while they are both capable of restraining the preternatural activity of the glandular system of the alimentary canal, neither of them can be safely used after collapse has set in. The astringents which would be at once the safest and most effective in the treatment of cholera, would be those which would only act topically on the mucous membranes and glands of the stomach and bowels, and which could not be absorbed at all. If any such drugs can be



found they will, I believe, prove extremely beneficial ; for the more rapidly and completely that intensely active secretion which is the source of the evacuations can be arrested, the greater will be the chance of saving the lives of cholera patients, and of hastening their recovery. The medicinal desideratum still to be found—if happily it ever shall be—for the treatment of cholera must, however, possess the power of subduing the hyperæmia of the spinal cord and sympathetic ganglia without inflicting any injury on the system meanwhile ; for if my hypothesis concerning the disease be correct, it is obvious that medicines intended merely to suppress its symptoms will at best be only palliative and of comparatively slight use.

*Bromide of Potassium and Bromide of Ammonium.*—The precise mode of action of these salts is not known ; but what is known of their peculiar influence on the nervous system would induce me to give them an extensive trial in the treatment of cholera if I should have an opportunity of doing so. There is good reason to believe that bromide of potassium so affects the nervous tissue as to lessen its power of attracting blood to itself. At all events it is certain that in the treatment of the whole group of convulsive affections, the chief proximate cause of which is spinal hyperæmia, this drug is more efficacious than any other of the countless number which has been tried in these diseases. It is therefore reasonable to suppose, if the proximate cause of cholera be what I allege it to be, that bromide of potassium will exert a curative influence over it. Bromide of ammonium seems to me, so far as I am able to form an opinion of its action, to counteract the tendency to congestion, and to facilitate the capillary circulation, especially in the nervous system. If so, it is likely that it will prove useful in cholera cases by facilitating the re-establishment of the circulation throughout the body. For the same reasons which lead me to give these salts together in epileptical affections, I should expect more benefit in cholera cases from their use in combination than if given separately.

*Quinine.*—The analogy between ague and cholera and the specific effect of quinine on the former has given rise to the suggestion that it is likely to exert a beneficial influence on the latter. But though there is an analogy there is also a great difference between these diseases ; for, as I have shown, the essential nature of ague consists in instability of the circulation of the blood in the nervous centres, whereas in cholera there is hyperæmia of those centres throughout the whole period during which collapse continues. It is true that this difference is only of degree in respect to the length of time during which the cold stage continues ; but, practically it is so great as to necessitate a difference of treatment, when medicines are used,\* not only in degree but in kind. I have

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\* I say "when medicines are used," because my method of treating the two diseases would be *essentially* the same, viz., by means of cold along the spine during the cold stage, and of heat during the hot in both cases.

given much attention to the therapeutical action of quinine, and am of opinion, that it mainly consists in so modifying the nature of the constituent elements of the nervous system as to cause them to attract blood to themselves more copiously and more steadily than before, and that while there is a certain analogy between opium and quinine, in respect to their effects, the latter acts less vehemently and more persistently. I know that it is capable of first increasing and then decreasing the amount of blood in the brain, and of causing sickness to the extent of vomiting; and though I have not yet been able to verify the fact, I have reason to believe, and venture to predict, that quinine will be found to increase the secretion of urine. Assuming that quinine exerts its influence in this manner, it is intelligible how it should prove the specific it is in ague;\* but I can scarcely think it likely that it will be of use in the treatment of cholera after collapse has fairly set in; in the earlier stages, however, I should rely upon it with greater confidence than I should upon opium, and hence I regard it, in combination with sulphuric acid, as the best medicine known for choleraic diarrhœa.

*Arsenic.*—Considering the remarkable power of arsenic in promoting the peripheral or capillary circulation, I should expect, *a priori*, that in the treatment of cholera it would prove one of the most beneficial medicines, and I observe in the *Lancet* of August 25th, 1866, that Dr. Black of Chesterfield, says, “I have now had great experience in the treatment of cholera, and that experience leads me to the conclusion, that if the treatment of the disease by the liquor arsenicalis be begun before extreme exhaustion has been produced, very few cases, however severe they may be, will terminate in

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\* If quinine cures ague in the manner suggested in the text, it does so, of course, most effectually when taken in frequently repeated doses so as to keep a constant and regular supply of blood in the nervous centres—the contraction of the cerebral arteries, which would be an *ultimate* result of one *large* dose of the drug, being prevented by each succeeding dose which again produces an *immediate* increase of the amount of blood in the brain. Singing in the ears when caused by quinine denotes, I apprehend, that hyperæmia of the ganglionic nervous centres has become predominant and is resulting in contraction of the cerebral arteries and, consequently, in anæmia of the brain. If this view be correct each fresh dose of quinine will, up to a certain period, subdue this symptom by again attracting a fresh flow of blood to the brain. But there may come a time when the sympathetic ganglia are rendered so extremely hyperæmic by the quinine that the consequent anæmic state of the brain cannot be overcome by fresh doses of the drug. Then singing in the ears will persist until the excessive quantity of quinine has been got rid of by excretion, and is an unmistakable signal that no more should be given.

If the *modus operandi* of quinine here explained be the real one, the question arises,—Is not its influence on the nervous centres such as *ultimately* to increase the mobility of their vascular structures, and therefore the instability or irregularity of their blood-supply? And if so,—though quinine cures ague, does it not actually increase the liability of the organism to fresh attacks, whether it be exposed anew to malarious influences or not?

death." Dr. Black believes cholera to be the product of an organic poison; he alleges that arsenic is an antidote in the majority of cases to the poison of venomous serpents, and infers, therefore, that it must be an antidote to the "poison" of cholera. Though his assumption and reasoning may be fallacious, his facts may be good, but they will need the verification of a large experience before that great curative power can be accorded to arsenic which he claims for it.

*Nitrate of Amyle.*—This substance possesses a very remarkable quality, which has led me to hope that it may prove of great value as a remedy for cholera. If the tip of a very small feather be dipped in it, and then held to the nostrils for two minutes, the circulation of the blood is rapidly increased to an astonishing extent: a sense of extreme fullness is experienced in the head; the face is suffused with blood, and becomes extremely red and swollen, so as to produce a wonderful bloated appearance within two or three minutes; and, simultaneously with the increase of the heart's action, there is a feeling of a general increase of heat and of embarrassing plethora. Shortly after the feather is withdrawn from the nostrils the symptoms begin to subside, and in a few minutes have nearly passed away. I have experimented upon myself twice with this substance, and my son has done so once in my presence: the symptoms on each occasion were the same.\* The nitrate of amyle used was not diluted but pure. It is probable that it would be more expedient and safe to experiment with it when diluted with say twenty times its bulk of alcohol. Its remarkable property of increasing the circulation in the rapid and astonishing manner described causes me to think that it gives greater promise of acting as an antidote to cholera than any other medicinal substance yet known.

I much regret that as yet I have had no opportunity of trying its efficacy, and hope that those physicians who have charge of cholera patients will carefully test its value.

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\* It is some months since these experiments were made, and I now write this report of them from memory. Much more careful and extensive observation on the physiological action of nitrate of amyle are still needed. The specimen of it, and the only one I have seen, was made by my son, Ernest T. Chapman, who has suggested that it may prove useful as a remedy for sea-sickness. There is one great objection to it: it has a very strong and disagreeable odour.

## CHAPTER VI.

## CASES AND RESULTS.

CASE I.—I was summoned in the morning of March 14, 1865, to see Mrs. D. Just as I reached her room at 7.30 A.M. she had been got up to the night chair. Immediately she rose from the chair she stood still and speechless, and anxiously beckoned to her attendant, who rushed to her, and caught her as she was falling. She was got on to the bed, where she lay for some time as if dead. The window having been opened she was raised and held up, when she soon recovered from what seemed to be a fainting-fit. At 8 A.M. she had another motion, and experienced such extreme dyspnoea, that she appeared to have quite lost the power of breathing. She gasped; her countenance rapidly changed—became quite dark coloured and remarkably pinched and contracted. Her hands were purple and icy cold. She made a violent effort to speak, and succeeding at length in doing so, shrieked out: "Lord save me! Lord save me!" At last after struggling for some time, when I feared she would sink, she began to breathe more freely again. She informed me afterwards that she had found that when troubled with nightmare, as she often is, a vehement effort to scream has delivered her from the oppression, and hence her shriek on this occasion. The attack began two days previously with diarrhoea, which had continued ever since. During the night of the 13th the patient was purged several times, became alarmingly weak, and suffered much from cramps in the abdomen and lower extremities, which quite doubled her up.\* While I was with her, between 8 and 8.30 A.M. she was agonized with cramps, especially in the thighs, and vomited repeatedly during the morning. I ordered a dose of chalk mixture at once; but it was speedily ejected.

Not until 8.30 A.M. was it possible to procure ice. I applied it immediately, *and within five minutes she was in a placid sleep!* She slept forty-five minutes, and was then woke up by the leaking of the ice-bag. She had another motion immediately—nothing but a serous-like fluid, with flakes of mucus floating in it—a true "rice-water stool."

As she was menstruating, and, only a few days before, had had a

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\* By a strange inadvertence I omitted to mention this symptom in my report of this case, published in the first edition of this work.

severe attack of hæmoptysis, I dared not use the ice freely and continuously along the whole back, as I otherwise should have done. But it was used so far as was necessary to keep the vomiting subdued; and whenever the vermicular movements and cramps of the bowels, which especially distressed her, became distinct and considerable, they were arrested by applying ice for short periods at a time in the dorso-lumbar regions.

Thus the malady which, owing to the state of the lungs and womb, could not be suddenly cut short, was gently and gradually subdued. But the patient continued fearfully weak during the 15th, 16th, and 17th, although the choleraic symptoms were completely controlled. She was a high-spirited, strong-willed person, who had an insuperable objection to lie in bed while her bowels were moved, and hence being extremely weak, almost every time she was raised up or her bowels were moved, she became unconscious, and often lay as dead. Several times I was suddenly summoned when her attendants were helpless, and frightened lest she was on the point of death. On one occasion I found her lying on the floor of the room, pallid, unconscious, with a pulse scarcely perceptible, all the muscles relaxed,—a dead weight, which the three women surrounding her could not lift into bed.

*Comment.*—The treatment of this case was peculiarly difficult: much blood had recently been lost from the lungs, and was still being lost from the womb by excessive menstruation; ice along the whole spine continuously would probably have caused the hæmoptysis to recur, and would certainly have increased the menstrual flow; hence, though ice, freely applied, would have promptly arrested the choleraic symptoms, and would, by increasing the flow of blood in the peripheral arteries, have prevented the frequent fainting fits which occurred, I could only use it safely during a few minutes at a time.

[Cases II. to VIII. inclusive were kindly placed in my hands for treatment either by Dr. Cheeseman or by Mr. Bencraft, surgeon, of Southampton, during the month of October, 1865. The reports of these cases are reprinted from *The Medical Times and Gazette* of October 14th, 1865.]

CASE II.—Mrs. F., aged 29, eight months pregnant, living in one of the worst parts of Southampton, was first seen by me on October 4, 11.30 A.M. Has had diarrhœa about ten days. On the 2nd inst., in the afternoon, felt faint, and at 7 P.M. took to her bed. Had previously begun to be purged “dreadful.” Since then has been purged continually every ten minutes, or oftener. Cramps have occurred at intervals during the ten days, but on Monday night they became very bad, recurring continually, has not had five minutes’ sleep at a time for them; skin cool, but not very cold. Ate an egg this morning, and, as her first act of vomiting, has just

thrown it up. No headache, but is very giddy; cannot stand one moment. Head decidedly cold; eyes deeply sunken; tongue cool. "Feels" (she says) "tight in the chest, and hot in myself." Very slight pain in bowels; passes water. Pulse 108, thin, and very feeble. Applied ice at 11.45 A.M. along the whole spine, and ordered it to be removed at once if any hypogastric pain were experienced. 2.45 P.M.—Was soothed during the first forty minutes, then began to have pain in the region of the womb, but continued the ice; cramps much lessened; bowels moved but twice; has been sick twice; pulse 104. Continue the ice every other half-hour, and give beef-tea. 7.15 P.M.—Bowels moved twice; sick twice, but retained the beef-tea. Cramps have subsided, but still has pain in the hypogastric region and down the thighs; is very comfortably warm all over. To apply the ice only between the scapulæ every other half-hour as before: to remove it at once if internal pain is induced. 11.15.—Has slept several times during the evening. Hypogastric pain continues; bowels moved once; sick twice: continue as before.

5th, 8.30 A.M.—Bowels moved twice; sick four times, twice provoked by medicine prescribed before I saw the patient, and given without my knowledge. Pulse stronger; warm all over; no cramps. Still has dull pain in hypogastric and lumbar regions; nothing, however, of the nature of spasms. Has retained her beef-tea, and says the ice comforts her. To reapply it along the whole back, until it melts, unless hypogastric pains increase. 5 P.M.—Is altogether better; pain lessened; no cramps; warm all over; bowels moved but once; stool decidedly fecal; has vomited a little after drinking; sleeps often, and feels rather heavy in the head.

7th, 9.30 A.M.—Used ice three times since last evening. Has been sick three times. The bowels have not been moved since yesterday morning. Still warm; no cramps; pulse 104; has eaten two eggs and some fish. To use ice only if coldness, cramps, sickness, or diarrhoea should recur. To eat anything she may fancy. 10 P.M.—Has been much better all day; neither sickness nor movement of the bowels. Is now asleep.

8th, 9 A.M.—In the middle of the night began to "wander," and got up. Does not answer questions coherently this morning. The eyes have a peculiar aspect, as if expressive of cerebral oppression. Head, chest, and extremities warm; pulse rather strong and rapid. Has been neither sick nor purged. Loud bronchial breathing, but no respiratory murmur at back of each lung; breathing rather laboured. I *thought* the breath had a diabetic smell. Still no movement of bowels. To discontinue the ice, and to apply heat between the scapulæ, and to renew the warm water in the bag every hour. 4 P.M.—The heat gave immediate relief, both to the head and chest. In a few minutes after its first application she became quite coherent, broke out in a perspiration, and felt much better. Has had a long sleep—the longest since she has been ill. Says, "I'm wonderfully better; I only want a cup of tea to be all right

now." Still neither vomiting nor purging. To renew water-bag at intervals of ninety minutes.

9th, 9 A.M.—Feels a great deal better in herself; slept two or three hours; still no sickness and no movement of the bowels; passes urine freely; skin cool; pulse 112; tongue coated. To continue water-bag once every two hours, and to have a saline mixture. 4 P.M.—Still better and stronger; has slept much; tongue cleaner; no sickness; bowels still unmoved. Says she feels now that she only wants plenty of good food. On this occasion I ordered the use of the water-bag to be discontinued, and took my leave.

*Comment.*—As this woman is pregnant, peculiar care and patience were needed in her treatment by means of ice, otherwise a miscarriage must have been induced. Foreseeing before ice was first applied the danger of its prolonged use at one time, I ordered its removal at once if hypogastric pains were caused by it. It will be seen that my precautions were needful, but that by careful management such tolerance of the ice was ultimately established as to enable me to overcome all symptoms of her malady. The experience of the effects of the heat in restoring sanity, inducing sleep, causing perspiration, and relieving the lungs is not less striking than instructive.

CASE III.—Mrs. L., aged 29. First seen October 6, at 4 P.M. Began to have diarrhoea and vomiting at 6 A.M., stools being yellow. Cramps began at 10 A.M.; at 11 A.M. the skin became cold and discoloured. Symptoms gradually increased. At 4 P.M., when seen by myself, Mr. Bencraft, and Dr. Welsh, she exhibited the choleraic countenance in a very striking degree; eyes deeply sunken; lips blue; the whole surface of the body cold; cramps violent; rice-water purging and vomiting. Ice ordered to be applied continually. 10 P.M.—Decidedly improved. Markedly warmer; cramps and sickness much lessened. The cramps only recurred when the ice had melted and the bag had become warm.

October 7th, 10 A.M.—Aspect and voice much improved; lips red; whole surface of body quite warm; pulse 100. Cramps only occurred once—when the bag had become warm. Bowels moved three times; vomited once. To continue ice as before. 8 P.M.—Body nice and warm. Pulse 94, fuller and stronger; slightly sick once; no cramps; bowels moved twice; the stools have a faecal smell. Very thirsty and weak; indisposed to take nourishment. Had some beef-tea and milk, and kept it. To apply ice till it melts, then omit it for half an hour, then repeat it in the same way.

8th, 10.30 A.M.—Has had rather a restless night, wanting sleep, and troubled with hiccough. Warm all over. Pulse 90. No vomiting; bowels moved but once; no cramps. To apply warm water-bag along cervical and upper dorsal region, and to renew it every two hours. R. Ferri et quinae citratis, gr. v, ter die. 11 A.M.—Called again and found her asleep. 4 P.M.—Found she had slept



about an hour—twenty minutes on three different occasions. Bowels moved once. To have tea and milk. 9 P.M.—Feels and looks much better; comfortably warm all over; pulse, as Mr. Bencraft said, “capital”—84; no vomiting or cramps throughout the day; bowels moved once. Enjoyed her tea, milk, and sago. The face is looking clearer. Now she expresses a fancy for food—viz., ham. To apply ice in lumbar region at 2 A.M. to-morrow.

9th, 11 A.M.—Ice was not applied as ordered. Has had a restless night, and between 4 and 6 A.M. had four motions, with copious amount of bile in them. Is generally warm; pulse 72. Complains of headache if she rises up. Tongue rather darkly furred on dorsum, clean at edges. Has had neither sickness nor cramp. Has taken half a cup of tea with milk; could not swallow any meat. To apply ice in lumbar region thirty minutes every four hours; but if diarrhoea returns to apply it continually. 8 P.M. (Report from Mr. Bencraft).—“Mrs. L. is doing well. Pulse 72. She is not so restless or feverish; has taken some tea with milk, two good cups of beef-tea, and a fruit tart which she fancied.”

*Comment.*—It will be observed that on the night of October 8th, I ordered ice to be applied at 2 o'clock the following morning, although both vomiting and purging had ceased. I did this because as patients are most frequently attacked with cholera early in the morning, so will they be most liable, as it seems to me, to relapse at that time. There being no ice in the house on the night in question, none was applied; and, as I anticipated, diarrhoea recurred. The power of the warm water-bag in inducing sleep is exemplified in this case quite as strikingly as in the previous one.

CASE IV.—Ellen S., aged 15, first seen October 6, 12 P.M. Began to menstruate five months ago. Of late years has had pretty good health, but has always been delicate in the chest; eight years ago had a severe attack of diarrhoea and “slow fever.” This morning, at 7.30, had a stool, and complained of pains in her chest and stomach. She continued to be purged and to vomit at times, and at 4 P.M. assumed the ghastly cholera aspect, and said she thought she was going to die. Since then the purging and vomiting have been almost incessant. She is very cold all over, the wrists and tongue especially so; the head alternately cold and hot. Pulse almost wholly imperceptible; cold perspirations; very bad cramps; rice-water stools. When first seen the peculiar choleraic countenance was more striking than I had seen before: it was appallingly deathlike. Had “a bilious pill” at 10.30 A.M., and pill of calomel and opium (gr.  $\frac{1}{4}$  of the latter) at 11.30 A.M. I ordered ice to be applied to the whole spine every two and a half hours.

October 7th, 9 A.M.—Is strikingly better: warm all over; tongue considerably warmer; pulse very distinct, 116; head comfortable; countenance immensely improved; no cramps at all; has vomited but once. The fæces, which are still passed under

her, have a distinct fæcal smell. Has had a cup of tea. Ordered to apply ice during an hour; then to omit it half an hour; then to resume it for the hour, and so continue. To have beef-tea as often as she can take it. 2 P.M.—Continues warm; tongue warm; no cramps. Has been sick once, at 9.30; not since. Bowels not moved since 11 A.M. Has dozed a little; no headache. Has had half a cup of beef-tea, and has kept it. 8.30 P.M.—Thinks she does not feel quite so well. Warm all over, except the feet; pulse 104; no cramps; sick once; bowels moved once; stool fæcal. Has had two half cups of beef-tea, and has kept them. Had the ice on every alternate hour. To continue the ice an hour, omit half an hour, and repeat continually in this way.

8th, 10 A.M.—Is still warm all over, especially the chest. Pulse 116. Has been sick and purged ten or twelve times during the night. The last stool was quite of the rice-water character, and contained a large round worm, which was dead. She complains of pain in the chest, and I cannot hear respiratory murmur at the lower part of the back of the lungs. Has recently slept about half an hour. To apply ice to lumbar region only, and continually. To apply warm water between scapulæ every hour. 4 P.M.—Found her asleep. Feels easier in the chest; vomiting and purging much lessened; stools have strong fæcal odour. Has had more beef-tea. Continue. 10 P.M.—Still better; has slept an hour; pulse 100. Mr. Bencraft, who saw her with me, is struck with the improvement in her appearance. To use the ice as before, and the warm water every two hours.

9th, 10 A.M.—Has had rather a restless night, and complains of pain in the bowels. Pulse 92; breathes comfortably; sickness at intervals. To discontinue the application of the warm water-bag, and to apply ice continually in lower dorsal and lumbar region only. 4 P.M. (Mr. Bencraft's report).—"Sickness lessened; bowels moved twice. Still restless and complaining of pain in the stomach." Mr. Bencraft, who kindly promised to keep a record of the further progress of this patient after I left Southampton, subsequently informed me that she improved steadily and completely recovered.

*Comment.*—As this patient has a weak chest, it was necessary to watch carefully the effects of the ice, and to apply it as symptoms suggested. Hence it was that although she had been much troubled with both sickness and vomiting during the night of the 7th, I, next morning, restricted the ice to the lumbar region, and ordered heat to be applied between the scapulæ. It seems to me not improbable that in the preternaturally sensitive and excitable state of the bowels the dead worm which she passed caused the purging to recur. In this case, again, the warm water-bag not only relieved the chest, but speedily induced sleep. I must insist emphatically, however, that heat can only be thus used advantageously in cholera cases when great care is exercised, both in respect to the length of time it is applied and to the temperature of the water used, as heat, even between the scapulæ, tends to prolong the sickness.

CASE V.—Mr. B., a strong man, accustomed to work at the docks, was attacked on the morning of October 8. I saw him with Mr. Bencraft in the evening of the same day, when he complained of incessant vomiting, purging, and cramps, and evinced great anxiety and distress; but though the skin was rather cool, it was not cold; and as, therefore, no marked algide symptoms were developed, the case was not thought to exhibit the characteristic symptoms of cholera in a form so decided as to be a desirable one for testing the efficacy of my method of treatment. He was therefore treated by medicine only.

October 9th, 11 A.M.—I was again requested to see the patient, the algide symptoms being now very marked. Indeed, all the characteristic evidences of cholera were present; but just before I reached the patient, Dr. Lake had applied an ice-bag along the spine. I did not see this patient again. Dr. Bullar, however, who called upon me the same day just as I was leaving Southampton, informed me that by twelve o'clock a favourable change in the patient was observable, and that he was already becoming warm; and I have since received a note from Mr. Bencraft, dated 9 P.M. the same evening, in which he says:—"B—— is much better, warm all over, a capital pulse, but still sick; no cramps." He quite recovered.

CASE VI.—Mrs. T., aged 42; first seen October 4, 11.30 A.M. Has been suffering from diarrhoea for several days. On the 29th ult. she suffered much from cramps. Yesterday she was very ill with diarrhoea, and this morning her bowels have been moved six or seven times. There is no faecal matter in the stools, which are wholly of the "rice-water" type. Complains of giddiness and deafness. Skin generally cool; legs and feet cold. Ice was applied at once. 3 P.M.—Feels better, and has had but one motion. 7.30 P.M.—Has felt inclined to be sick, but was not so. Had two motions, but no longer of the "rice-water" kind. Has eaten fish, and kept it. To continue the ice. 11 P.M.—Head and upper extremities very warm; lower extremities cool; pulse 80, intermittent. Has vomited once, provoked to do so by medicine prescribed before I saw her, and which I did not intend her to take. Had one motion. To apply ice in the two lower segments of the bag only.

October 5th, 8.30 A.M.—Is warm all over except the feet. Pulse 80, intermittent. Has not been sick; bowels moved three times, but the stools more substantial; feels weak; is taking ground rice boiled in water, grapes, and preserve. To apply ice every four hours until the ice melts on lower third of spine only.

6th and 7th.—This patient progressed satisfactorily, but during the night of the 7th no ice was used, and early the next morning she was purged three or four times.

8th, 12 A.M.—Has eaten an egg and had warm milk; is fairly warm all over; no cramps, and the last stool recently passed is semi-solid. To continue ice, as last ordered, three times a day, and at two o'clock in the morning. R. Ferri et quinqu. citratin, gr. v, ter die.

9th, 9.30 A.M.—Has had a considerable amount of sleep, but about 2 A.M., before ice was applied as ordered, the patient was violently sick, and purged several times; no cramps; skin nicely warm; pulse 80; tongue thickly coated in parts, and beefy-red in others. Has had an egg and half a pint of new milk this morning. To continue the ice along the lower half of spine, and to take a saline mixture. 4 P.M.—Patient feels much better, and is in good spirits, both she and her husband believing that she will soon be well. Having given such general directions as I thought desirable, I took my final leave. She completely recovered.

*Comment.*—The experience in this case again justifies the belief that the liability to relapse is greatest shortly after midnight, and that it is especially necessary to take precautions to avert this result by applying ice immediately after midnight at all events, until the patient is fairly out of danger. The more cholera is studied, the more it will be found, I believe, that the violence of the disease is exhibited in different patients in different parts of the body: in one the skin exhibits the most marked symptoms, in another the voluntary muscles, in another the stomach, in another the bowels; and in hot climates the head is often so suddenly and powerfully affected as to result in death from coma almost immediately after the attack. The patient whose case has just been described has been a long sufferer from deficient and painful menstruation, and from coldness of the feet—facts which denote that the lower half of the nervous centres along the back have long been in a state of chronic congestion. Hence it was to be expected, as experience has proved, that the choleraic attack would chiefly expend itself on the bowels and lower half of the body. For the first time during a long period the feet of this patient have become permanently warm.

CASE VII.—Mrs. B., aged 46, widow, residing in one of the very worst parts of Southampton, began to be ill at 1 A.M. on the 2nd inst. Was attacked with constant sickness and purging; became cold all over, with cold sweat-drops “as big as peas,” and her skin assumed a purple hue; was troubled with cramps of the extremities, the fingers becoming quite stiff. The same evening (the 2nd), Dr. Cheeseman wrapped her in a wet sheet, which seemed to lessen somewhat the coldness of the surface and to refresh her.

She was seen by me for the first time on October 3rd, at 9 P.M., when Dr. Cheeseman kindly allowed me to treat her. At that time the vomiting, purging, and cramps, which had also invaded the chest and abdomen, persisted undiminished. The muscles of the extremities were hard, somewhat board-like, showing them to be in a state of tonic spasm. The head was alternately hot and cold. The upper part of the chest was decidedly cold; the precordia and epigastrium warm. Arms and legs cold, the wrists especially so. She complained much of her chest and head. No urine had been passed during the 2nd and 3rd inst. At 9.50 P.M. I applied ice

along the whole spine. At 9.55 the hard muscles had become soft and flaccid. This fact was also witnessed by Dr. Cheeseman. At 10 P.M. she fell asleep, and excepting an interval of ten or fifteen minutes, slept an hour and a half.

October 4th, 1 A.M.—The upper part of the chest had become warm, and the extremities slightly warmer. Vomiting and purging lessened. 11 P.M.—Has vomited only eight times since last visit, and the bowels have been moved but four times, and not at all since 3 P.M. At 7 P.M. had become warm all over and looks less distressed. The cramps have almost wholly ceased to trouble her. The bronchial tubes are much clogged with thick mucus, but the quantity expectorated is already lessened. Am now told for the first time that she began to menstruate this morning, although she only ceased a week ago.

October 5th, 5 P.M.—Complained of much pain of chest and “kidneys” during last night. Bowels have not been moved since 3 P.M. yesterday; has been sick six or seven times; ejects bile. The skin is warm all over. An injection of beef-tea given to-day has been retained. To apply ice between the scapulæ and along the lower cervical vertebræ just sufficiently long to control the sickness; the legs, which have become cooler, to be kept warm by clothes and hot-water bottle. Although the algide symptoms of this patient were completely overcome, and the diarrhoea had wholly ceased, and although the last stools which were passed were quite of a faecal character, and of considerable consistence, she ceased to improve, and finally sank in the evening of October 7th.

*Comment.*—This patient was an habitual drunkard, is said to have lived on drink during the week preceding her attack, obtained gin early in the morning of the 5th, 6th, and 7th, and during the whole of the 7th, until about 7 P.M., was destitute of food, or the means of getting beef-tea, fuel, and ice. On the evenings of the 5th and 6th, I supplied a small sum, directing beef-tea to be purchased, but I fear that my little help was converted into gin. The nurse, who had been up several nights with her, whose apparently unremunerated devotion to her was a mystery to me, and who, no doubt, was wholly exhausted, was found by a physician lying drunk by the side of the patient. Of course, as there were no means of treatment in the house on the 7th, I avoided visiting the house until evening, when I found the patient in a dying state. Before my treatment began, the patient had had opium and calomel; forty grains of the latter.

CASE VIII.—Mrs. D., aged 73, married. October 6, 10 A.M.—Began to be ill last night with vomiting and purging, fearful coldness and cramps, and continued in that state until I saw her, when I ordered ice along the spine. 7 P.M.—Hands still cold; feet much warmer, also felt the warmth in the abdomen; has been purged twice and sick twice; has not had any more cramps. To continue the ice until quite warm all over.

7th, 9.30 A.M.—Has had five bags of ice since treatment began. Is warm all over; expression improved; has a little headache; has a little pain about the bowels, but no cramps; bowels moved three times since 7 P.M. yesterday, and has not been sick at all. Has had some beef-tea. To continue the ice every other hour. Food *ad libitum*.

8th, 11 A.M.—Thinks she feels a little better, but has been purged four times during the night, and is very weak; is fairly warm all over. To omit ice from upper cell of bag, continuing the application as before. To have some arrowroot. R *Ferri et quinz citratis*, gr. v, ter die.

9th, 10 A.M.—I was told by her husband that she was dying; found her breathing with extreme labour; the inspirations very forced and deep; pulse at wrist feeble; aspect deathly; eyes dim. Applied ten-inch warm water-bag; she revived considerably, began to breathe easier, swallowed a little brandy-and-water, was conscious, and answered questions by a laboured monosyllable. Gave her a teaspoonful of brandy twice; ordered it to be repeated at the end of an hour, the water-bag being continued and renewed meanwhile. She sank, however, in the course of the day.

*Comment.*—There is a sewer-grate close to the door of the patient's house, and the husband informed me that a short time since, when the stench from it was worse than usual, she said, "That sewer will kill me." A lady friend, who had taken an interest in them for years, told me that they had often been without food, and that frequently when Mrs. D. had visited her, and was given something to eat, she stealthily divided it, in order to take the half to her husband. It is obvious that her aged frame, already suffering from inadequate nourishment, had been too much shaken by the violence of the attack to live through the reaction following it.

In the report of Case VII, it is stated that the patient began to menstruate, although she had ceased to do so only a week before. The extraordinary power of ice, when applied along the spine, in increasing the circulation, even in parts of the body which have become more or less atrophied, was very unfortunately exemplified in the case of this old woman, aged 78, who began to have hæmorrhage from the womb on the day of her death! It is not improbable that this discharge exercised a baneful constitutional effect, and contributed to hasten the fatal end. Several cases have come before me in which eminent physicians, who have been consulted as to the safety of applying ice along the spine, have expressed the opinion that, at all events, it can do no harm. They will, I hope, see in this fact, how mistaken that opinion is, and with what extreme care this potent agent must be used.

Professor Parkes seems to doubt that the hæmorrhage in these cases was produced by the application of ice along the spine. In his letter published in *The Medical Times and Gazette* for August 25, 1866, he makes the following observations on the subject:—

"Much interest was excited last year and this year in Southampton by the fact of an uterine bloody discharge (like catamenial) following the application of Dr. Chapman's ice-bag to the spine in two or three cases. This seemed to harmonize with Dr. Chapman's explanation of the effect of cold to the spine on distant vessels. But I noticed in 1849 that the uterus was occasionally affected in cholera without the application of cold to the surface. In 11 adult women (several of whom died in the cold stage) there were no uterine symptoms in 6, but in the other five the condition was as follows:—In one woman the catamenia appeared three days after the usual time during the early diarrhoeal stage, after the patient, according to her own account, had had more than one hundred stools, and continued for about twelve hours during the early cold stage, then ceased. The patient died in reaction, without their reappearance. In another woman, the catamenia, having appeared two days before the attack, continued, though scantily, through the cold stage. In a third woman (aged 50) the catamenia appeared after an absence of nine months, at the commencement of reaction. In a fourth patient the catamenia, having naturally ceased three days before the attack, reappeared during reaction. In another case the catamenia appeared at the usual time during reaction.

"There is certainly no evidence here of bloody discharge from the uterus coming on during the height of the cold stage: but still the uterus is evidently affected by cholera, and it is very desirable to collect more facts before ascribing the catamenial flow which occurred in some cases at Southampton to the ice-bag."

It will be seen that in none of the cases here mentioned does uterine hæmorrhage or menstruation begin while the patients are in the algide stage; whereas in both of my cases the flow began in that stage, and in one of the cases the woman was 73 years old. Dr. Parkes is of course quite right in maintaining a sceptical attitude in respect to my allegation of the cause of the flow, and I am not surprised at his difficulty in believing that ice along the spine will induce uterine hæmorrhage in cases of choleraic collapse. But if he had acquainted himself with the facts reported in my pamphlet on "The Functional Diseases of Women," he would be aware that, at all events, ice in the dorso-lumbar region will produce or increase the menstrual flow in women not suffering from cholera, and would then perhaps find less difficulty than he does now in believing that the prolonged use of ice in cases of cholera may produce a like result.

In the "Eighth Report of the Medical Officer of the Privy Council, with Appendix, 1865," Professor Parkes has given an official account of and a comment upon the treatment of the foregoing seven cases, as well as of two others, by ice at Southampton. It is right I think that his statement should be quoted here.



*"Curative Measures.*

"The short duration of the outbreak, and the small number of cases, render it impossible to give any opinion on the use of the various plans employed; the mortality, as already said, was considerable.

"Much interest was excited by the employment of ice-bags to the spine by Dr. Chapman, and various opinions were expressed as to the utility of this treatment. I saw several of the cases, and the impression made upon me was that the trial must be greatly extended before any decided opinion can be given. Ten cases were, I believe, treated chiefly or entirely with ice-bags, but in one case it was imperfectly done; 5 died, or 50 per cent; or excluding the case in which the trial may not have been fair, 4 died out of 9, or 44·4 per cent. But several of these cases were mild in type, and in two of the cases of recovery which I saw the symptoms were very slight indeed. The bags appeared to lessen the cramps, but not, I think, to diminish either the vomiting or the purging, and to have little, if any, effect on the algide symptoms. I ought to state, however, that two or three medical men who saw more of the treatment than I did, rank its utility much higher. Certainly it ought to receive a fair trial."

Of the ten cases here mentioned by Dr. Parkes, seven are reported by me above, two others, as I have been informed by Dr. Griffin, were treated by him at Freemantle, the remaining one, making the tenth referred to by Dr. Parkes as the one case in which ice was imperfectly tried, was, I believe, that of Mr. Cooper, the late medical officer of health of Southampton. But an eleventh case, which recovered under the care of Dr. Griffin, is mentioned by Mr. Ben-craft in his letter of Oct. 29th (see p. 224). It will be observed that five out of the seven of the cases treated by myself recovered, and that the two patients who ultimately died had been rescued from collapse. Dr. Griffin has informed me that his two patients at Freemantle were also recovered from collapse. He says that in consequence of their distance from him, the application of the ice in both these cases was continued too long, and expresses his opinion that if they could have had adequate attention the result might have been different. In respect to the treatment of Mr. Cooper, I received from a professional witness of it the following statement:—"He was prejudiced against the ice strongly. Ice-bags were certainly applied to him, but he would not allow that, or indeed any other plan of treatment to be persevered in. I saw him, and I believe that if ice had been properly applied his life would have been saved."

It is clear that in considering the results of the method of treatment introduced by me at Southampton in 1865, the case of Mr. Cooper cannot be taken into account. It will probably be admitted, by all impartial persons, that the recovery from collapse of every one of the nine cases treated by means of ice, is a significant fact.

Moreover, if the circumstances of the four fatal cases be examined, the reader will, I think, see strong reasons for believing that with proper treatment, nursing and nourishment, the number of deaths would have been still less than they were. However, if Dr. Griffin's two cases at Freemantle be included, it appears that 4 died out of 10, or 40 per cent. Now the total number of cholera cases in Southampton and its neighbourhood during the epidemic of 1865, was 60, the total number of deaths 35, or 58·33 per cent. But if the 10 treated by ice be omitted, the remaining numbers would stand thus: cases submitted to ordinary treatment, 50, deaths 31, or 62 per cent. The actual results therefore are numerically greatly in favour of my method of treatment.

Dr. Parkes states—"But several of these cases were mild in type, and in two of the cases of recovery which I saw the symptoms were very slight indeed." It is, I think, fair to presume that whatever may have been the degree of severity in any one of the cases in question, their average severity was quite as great as that of the cases submitted to ordinary treatment with which they are compared. Indeed, I have reason to believe that they were on the average more severe; for there was a feeling on the part of the medical men who allowed me to treat cases, that only those which were severe enough to be satisfactory tests of the validity of my method should be submitted to it. In proof of this, I refer to the report of Case V. already given. When this patient, Mr. B., was first seen by me, in company with Mr. Bencraft, on the night of October 8th, he had been suffering all day, and at that time "complained of incessant vomiting, purging, and cramps, and evinced great anxiety and distress," but though the skin was cool, there was not the deathly coldness characteristic of collapse: he was therefore treated by medicine only. Not until the next morning, when, in spite of the treatment adopted, the algide symptoms had become very marked, was he submitted to my treatment. Of Case II. it is said, in Dr. Parkes' Report, "Sarah French was seen by Dr. Cheeseman, who has no doubt the case was one of unequivocal cholera." Cases III. and IV. were certainly cases of complete collapse. Case VI. was the least severe: there was no collapse, but there were cramps and rice-water evacuations, and the woman had long been in a cachectic condition. It thus appears that of the five cases of recovery, three were in a state of severe collapse; one was slightly collapsed; and one, though not collapsed, was an unfavourable case.

Dr. Parkes says—"The bags appeared to lessen the cramps, but not, I think, to diminish either the vomiting or the purging, and to have little, if any, effect on the algide symptoms. I ought to state, however, that two or three medical men who saw more of the treatment than I did, rank its utility much higher." Fortunately I am able to record what were the exact impressions made upon the medical men to whom Dr. Parkes refers. Shortly after I left

Southampton, in the autumn of 1865, I went to Paris, hoping to obtain there further opportunities of testing the efficacy of the therapeutical method in question. Thinking that an expression by the Southampton medical men who had witnessed my treatment there, of their opinion of it, might aid me in drawing professional attention to it in Paris, I asked Mr. Bencraft to send to me there his written opinion as well as that of the other medical men of Southampton, who had seen my treatment of cholera there, and who might be disposed to state what they saw. While I was in Paris, I received from Mr. Bencraft the following letters :—

Southampton, Oct. 20th, 1865.

DEAR SIR,—Having given Dr. Chapman's treatment of cholera, by applying ice to the spine, a trial in those cases of that disease which have fallen under my care during the past two or three weeks, I have much pleasure in testifying that it has appeared to me one well worthy of a fair trial.

It proved itself a remedy of very considerable power ; restoring the heat, relieving the cramps, checking the vomiting and purging. Its use was followed by reaction from collapse, even in cases where the patient was quite pulseless.

As with all other remedies of power it requires to be used with discretion, and not continued too long.

This treatment appears to have the great advantage of producing reaction from the state of collapse, leaving the patient free from the very large quantities of medicines, which in other modes of cure so often fatally hamper the treatment of the secondary fever.

Although my experience of it has not been large, and it has not succeeded in every case, yet I think it deserves a careful and extended trial. Your obedient servant,

G. A. K. LAKE, M.D.,

Surgeon to the Royal South Hants Infirmary, &c. &c.

To Henry Bencraft, Esq.

Marland House, Southampton,  
Oct. 28th, 1865.

MY DEAR DR. CHAPMAN,—I am very happy to bear testimony as to the use of your ice-bags in the treatment of cholera. The five cases in which I had the good fortune of witnessing their application by you, have sufficiently convinced me of their utility in rousing the patients from collapse, and removing the algide symptoms of cholera. Should I, unfortunately, have any more cases, I shall strictly adhere to your treatment, and conscientiously recommend the medical profession to give it a fair and impartial trial. Wishing you every success, and with best regards, I remain, ever yours truly

G. CHEESEMAN,

Physician and District Med. Officer to the  
Southampton Incorporation.

Southampton, 26th Oct., 1865.

MY DEAR DR. CHAPMAN,—I have been desirous of testing as fully as I possibly can the worth of your ice-bag treatment in cases of cholera, this motive must plead my excuse for not writing to you before.

I have now seen and treated, with Mr. Bencraft and Dr. Cheeseman, six cases of the disease in question, in the stage of collapse; and what I have witnessed and noted in these cases, justifies me in stating that your treatment is superior to any that I have hitherto seen practised or pursued by myself or others.

This fact is unexceptionable—indeed, incontrovertible—that by all other methods, presumed or real, the cases have had a fatal termination.

In two cases treated on your principles by Mr. Bencraft, the cures are complete. One treated in a similar way by Dr. Cheeseman is a great success.

Based as your treatment is, on sound physiological principles, it deserves a fair and impartial trial; but all your injunctions must be strictly carried out.

The system of boiling patients in mustard and water is too puerile and brutal to do more than refer it.

Were I seized with cholera, I should give your mode of treatment a preference, indeed, I would submit to no other. Yours sincerely,

JOHN WIBLIN, Physician.

Southampton, Oct. 28th, 1865.

MY DEAR DR. CHAPMAN,—It gives me great pleasure to be enabled to add my testimony to those you have already received from Southampton, in favour of the beneficial results attending the treatment of cholera by means of your ice-bag applied to the spine.

Its power to relieve the vomiting, purging, and cramps, is almost marvellous; and its influence over the circulation, in restoring heat to the surface of the body, and bringing back the pulse where nearly and in some cases entirely gone, must be seen in order to be properly appreciated.

For my own part, I have such entire confidence in the method of treatment, that should any more cases of cholera come under my care I shall without hesitation trust to it alone; and were I to be attacked with cholera I would insist on being treated entirely by your method.

I may state that a *very large* proportion of the cases treated by ice in Southampton have made perfect recoveries, whilst almost every case that has been treated by other means has proved fatal.

With deep feelings of gratitude to you for having made me acquainted with your plan of treatment, I am, my dear Sir, very truly yours,

HENRY BENCRAFT, M.R.C.S.E., L.S.A.,  
Medical Officer to the Southampton  
Workhouse.

In a subsequent letter, dated October 29th, Mr. Bencraft adds—  
“I have no hesitation in recording my conviction, that if applied in anything like reasonable time, the ice will save every life; but I also see that it must be carefully watched. Dr. Griffin speaks very favourably of the ice treatment, and would employ it in any case that came under his care; but his experience has been too limited to give a testimonial. He had one bad case that recovered from it.”\*

In a lecture on cholera, by Professor MacLean, published in the *Lancet*, Feb. 3rd, 1866, he said, after referring to my method of treatment at Southampton:—“Dr. Chapman has since, I understand, had an opportunity of trying his method on a larger scale in Paris. I am quite prepared to hear that this more full experience has tended to lessen the confidence with which Dr. Chapman was inspired, when I had the pleasure of meeting him here.” This remark induces me to give a brief account of my visit to Paris, in so far as my treatment of cholera is concerned.

I reached Paris, October 14th, and immediately called upon Dr. Trousseau, to whom I had been introduced, and who, before I left England, had expressed his readiness to afford me any facilities he could at the Hôtel Dieu. Owing, however, to the restriction of the cholera patients to particular wards, he had none, because his wards were reserved for patients not suffering from cholera. He therefore kindly introduced me to his colleague, Dr. Vigla, and requested him to accord me the opportunities which he himself was unable to give. Dr. Vigla expressed his readiness to do so, and, conforming with the requirements of French hospital *régime*, took me to the office of the lay director of the hospital in order to introduce me in due form, and to give the assurance that he wished me to treat certain of his patients, and to have permission to enter the hospital at any time during the day. The chief of the office was not in; Dr. Vigla therefore left a message for him, to the effect just mentioned, with his subordinate, who intimated that he was sure it would be all right. Dr. Vigla then invited me to accompany him through his wards, and at once offered to place under my care for treatment a woman seemingly about 35 years of age, who was in profound choleraic collapse.

This patient had just been brought into the hospital, and had been subject to no treatment since her entry. She was extremely livid, and deadly cold; her skin was covered with clammy sweat; the eyes were deeply sunken; the voice almost extinct; she was quite pulseless, and in fact was wonderfully like a corpse. She suffered extremely from cramps, and the vomiting and purging were very frequent. I applied ice along her spine, between nine and ten o'clock in the morning, and fortunately the bag was well kept in its place. Warm water bottles were at the same time applied to her feet, and she was well covered with blankets as she lay. On re-

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\* This case, as stated at page 220, is not mentioned in Dr. Parkes's report.

turning to the hospital about two hours afterwards, I was assured that the cramps, vomiting, and purging had ceased; the patient certainly looked better, and the sister of charity in attendance voluntarily drew my attention to a change in the character of the skin of the patient which surprised her. The skin had become dry, more life-like, and seemingly somewhat less cold. She pointed out the difference between the skin of my patient and that of another in collapse in an adjoining bed, requesting me, by way of demonstration, to examine the latter.

While applying a second bag of ice, I was interrupted by the chief of the hospital, who at that moment entered the ward, and asked me in a very brusque tone what I was doing. I explained to him that I was there by invitation of Dr. Vigla to exemplify a method of treatment which had been successfully practised at Southampton. He said that no new experiments could be tried in that hospital; and, in reply to my remark that the treatment could not fairly be called a mere experiment, said that no new method of treatment could be permitted there unless first examined and sanctioned by a commission; and then, in an imperative tone, ordered me to remove the bag. I of course did so, and retired. The same afternoon I related what had occurred to Dr. Vigla, and made a similar communication to Dr. Trousseau. The next morning they remonstrated with the *chef*, who then politely informed me that there had been a misunderstanding, and I was again invited to treat the case. I was told, however, by the sister of charity, when I was going to the patient's bed, that, having heard the conversation between the *chef* and myself the day before, she had become so frightened by the idea of being a subject of mere experimentation that she would not allow my treatment of her to be resumed. She was therefore afterwards treated in the usual way, and soon died.

Another case, very similar to the above, but if possible of more profound collapse, was placed under my care by Dr. Vigla. This was a woman, also seemingly between thirty and forty years of age. She was treated in the same manner, and in a few hours the vomiting and purging were restrained, and the surface of the body was slightly improved. She was very restless, and having no ice-bag jacket, I found it impossible to secure the ice in its place along the spine during my absence from the hospital. On one occasion I found the bag across one side of the chest, and on another occasion I found the upper part of it lying in contact with the head. I was not admitted to the hospital between six at night and eight in the morning, and as there was only one night attendant for the ward, it was impossible that either the ice could be duly kept along the spine, or that heat could be adequately applied to the surface of the body. The woman died. Under these discouraging circumstances I did not ask Dr. Vigla for more cases.

I subsequently visited the Hôpital St. Antoine, and by invitation of Dr. Buchet, applied spine bags on the occasion of that visit to

four patients, all of them women. To the first one, who was in collapse, one bag of ice only was applied, and that much against her will. It exercised some restraining influence on the discharges, but, as she evidently imagined herself the subject of an experiment, the bag was not replenished after the ice had once melted. The second case was that of an old woman, so emaciated as to suggest the idea that she was in the last stage of phthisis; two bags were applied to her in succession. She repeatedly said, "*Il me fait du bien*," and evidently found it a great comfort. I cannot remember what other effects were produced in this case. The third case was one of very energetic reactionary fever. There was considerable cerebral congestion, causing heavy congestive sleep; the forehead as well as the rest of the body was flushed and hot, and the pulse was strong and rapid. I applied a spinal water bag, containing water at about 115° Fah. along the upper half of the spine. Within an hour the change for the better was very striking. The forehead became cool, a gentle sweat appeared upon it and upon the face, and one of the *Internes* of the hospital was the first to point out the fact that there was a very decided fall in the temperature of the surface of the chest. The fourth case was that of a woman who had been treated according to the method then adopted by Dr. Buchet, and which chiefly consisted in sponging the patient all over with water, and in then wrapping her up carefully, covering her with an abundance of blankets and applying hot bottles to the limbs. The measures adopted wholly failed to recover the patient from collapse. She lay in a state of profound stupor; seemingly there was no cerebral action at all. The head was thrown backwards, the mouth was open; when addressed in a loud voice she gave no sign of consciousness, and looked exactly as if dead. The head and upper extremities were deathly cold, the lower extremities were, however, slightly warm. She was quite pulseless. There was neither vomiting nor purging, and no evidence of cramps. I applied ice to the upper half of the spine only, and altogether three bags of ice were used. The treatment began about ten o'clock in the morning, and was continued until about four o'clock in the afternoon. At this time, the temperature of the upper half of the body had increased very decidedly, the head became warm, the pulse perceptible, and the mental power had returned to such a degree that the woman answered questions intelligibly and easily. I restricted the ice to the upper half of the spine in this case for the following reasons:—First, because as the woman had been extremely drained by the discharges which had occurred in the previous stages of the disease, and therefore had but little blood in her body, I deemed it expedient in view of her peculiar condition to concentrate all the spasm relaxing effects of the ice upon the head and chest; and second, because, inasmuch as there was some activity of circulation in the lower part of the body, it seemed to me that if I had placed ice along the whole spine,



and thus relaxed still further the arteries in that part, the afflux of blood would have become still greater there, and my chances of recovering the cerebral circulation would have been less than they were by the method I actually pursued. I know nothing of the further history or fate of this patient.

During my morning's visit to this hospital a subordinate of the lay director was present, and seemingly regarded my proceedings as of a very questionable character; at noon, when I again visited the patients, his manner assured me that I was an unwelcome intruder; and he then informed me that I must not enter the hospital again, except during the visit of Dr. Buchet, who would arrive at 4 P.M. I met him then, and was then told by the lay-chief of the hospital, that I could not be allowed to treat patients there without a special permission from M. Huisson, the director-general of the Paris hospitals. In face of the difficulties with respect to the nurses keeping the ice along the spine, the application of heat to the surface of the body, and the due nourishment of the patients, I did not think it expedient to apply to M. Huisson; and, therefore, having discontinued, at that time, the treatment of the patients in question, I did not visit the Hôpital St. Antoine again. Thus ended the trial of my method of treatment in Paris. It would have been impossible, as I found, to try it there fairly and effectually unless a ward and a sufficient number of nurses had been placed under my own direction, so that I might have been able to visit the patients at all hours, and authoritatively to superintend their treatment in every respect.

CASE IX.—This case is described in the following letter which I received at the end of July, 1866:—

“Calcutta, March 4th, 1866.

“SIR,—In the *Times* newspaper of the 11th October, 1865, I read an article bearing your signature on the subject of cholera, and its cure by *ice*. As I am in the habit of making notes, or preserving articles, that may possibly turn out useful some day or another, I cut out the article and inserted it among a number of other curious contributions to the press: at the time I little thought that it would turn out useful. I have a house in the country about fourteen miles from Calcutta, at a place called Tetaglan, close to the military cantonment of Barrackpore. On Sunday, the 25th February, I had a number of bullock carts coming from Calcutta; and finding that all had not arrived, I made inquiries and learnt that some had not arrived by reason of one of the cartmen having been attacked by cholera, but that some of the other cartmen had gone back to bring him on. At 10.30 the man arrived, and a worse case of cholera I have never seen—inconstant vomiting and purging—in fact, the latter was flowing from him. The pulse was scarcely perceptible, and the legs and arms quite cold. Voice almost entirely gone.

Medicine I had none. I had forgot to bring with me my small medicine chest which I generally carry. I sent one of my servants round to my neighbours for medicine, but did not succeed in getting any. At last I recollected your article on the subject of ice, and managed to procure some. At 11 A.M. I put a bag of ice on his spine from between the shoulder blades right down to the bottom. I used the leg of a pair of flannel trousers for a bag. The man was in great agony, and appeared to be sinking fast. I was determined, to the best of my ability, to give the ice a fair chance; so I sat on the ground by the patient and did everything myself. I had not had the ice on the man half an hour when I could perceive a change for the better. I asked the man whether he felt easier, and in a whisper he said yes. The cramps began to lessen visibly, and the involuntary purging to stop. He never vomited once from the time I applied the ice. After the ice had been on about an hour I found that the melting of the ice had made the cold a wet cold; and one of my natives suggested that I had better put the ice into a preserved fruit bottle, which I did, and dried the spine before I applied it. As I found the vomiting and purging had ceased, I put the bottle lower down the spine. I was much surprised about half-past twelve to find the pulse gradually getting stronger, and the legs and arms gradually warmer. I put my hand under the man's armpits and found a slight warm moisture. This warmth went on increasing until at half-past one his body, except where the ice was, became hot, as in fever. On this, at 1.30, I removed the ice from the man's back; and as I had to go away, I left orders with a trustworthy native, that in case the fever increased, he was to put a hot-water bottle between the shoulder blades. I returned again at 4.30 and found the man asleep, and a more ghastly object I never saw. A little before six he awoke, and I gave him about a pint and a half of congee, with a good wineglass of brandy in it, and well sweetened. This he drank with avidity; and having passed water, he fell asleep again almost immediately. At daybreak next morning I went to see my patient, and, to my surprise, I found him sitting up and smoking the everlasting hookah, which, rightly or wrongly, I took away from him, and made my servants make him a good mess of chicken broth, which he speedily demolished. About 9 A.M., as he was recovering fast, I questioned him as to his feelings on the day previous. He said he felt as if he was carrying three mounds (a weight equivalent to 240 pounds) on his back, and that he felt a burning pain in his stomach. That soon after he had the ice upon him, he felt his stomach getting cool, and the weight on his back became only two mounds, and that gradually the weight became less and less until it disappeared altogether. The above are the facts, and you may use them as you like. My letter will show I am not a medical man. I am a lawyer, and have had an experience in India of nearly forty years off and on. I have seen many, many cases of cholera, and many cures, and more deaths, but I never saw a case,

apparently so hopeless, cured so easily. The natives around me attributed the cure to *Jadoo*, or the evil spirit, and in that idea I have let them remain. In the evening of the following day the man's relatives all came from Calcutta to bury him, as they thought. He was taken away alive, but almost convalescent. I have sent the particulars of the case to the local journals, in which it has been inserted. As you ask medical men to publish the results of their experience, I send to you, as the discoverer of what seems likely to be one of the greatest discoveries of the age, the results of my experience, with permission to you to make what use of it you think fit. Allow me to remain, yours very obediently,

"A. T. T. PETERSON.

"Dr. John Chapman,  
"25, Somerset-street, Portman-square."

I have only to observe that in this case Mr. Peterson gave unremitting personal attention to his patient until recovery from collapse had been effected, and that afterwards his judicious use of heat in restraining the force of the reaction shows how thoroughly he appreciated the principles he was practising; while, being without spine-bags, the expedients he resorted to are at once creditable to his good sense, and are a valuable lesson to others in like circumstances. I avail myself of this opportunity of thanking him for the above very valuable and interesting letter.

*Cases of Cholera treated by means of the Spinal Ice-bag  
at Southampton during July, 1866.*

On Friday, July 10th, I received a telegram from Mr. Bencraft, Surgeon, of Southampton, as follows: "Plenty of cholera; using ice. Will you come down?" I started for Southampton the next day; passed a week there among cholera patients; was allowed to treat several of them; and obtained notes of all who had been treated either in the ordinary way or by means of ice by the four medical men who have either used ice or have requested me to apply it to their patients. I proceed to give a history of all the cases treated by means of ice, whether successfully or unsuccessfully, as I believe the history of the failures will prove even more instructive than that of the successes.

*Of the following cases, Cases 10 to 13 were under the  
care of Mr. Bencraft.*

CASE X.—Kate Legget, age 7, became ill on the night of July 13th, and was first seen by Mr. B. at 9 A.M. on the 14th, when she had all the symptoms of cholera, and was quite pulseless. Ice was applied continuously, and reaction was perfectly established by 2 P.M. of the same day; a good pulse having returned, she continued to do well, and has quite recovered.

CASE XI.—Mary Ann Gregory, age 12, was attacked July 12th, at 11 P.M., with vomiting and purging, and cramps in the legs, which continued to increase, and in the afternoon of the 13th she was collapsed and livid. The spinal ice-bag was applied at 1.30 P.M., the cramps and vomiting speedily ceased, and by 5 A.M. of July 14th the purging had ceased also. The ice was continued uninterruptedly till I saw her in the evening of this day. She was then warm all over, had a good pulse, and was seemingly out of danger. I advised that the ice should now be applied at intervals, and that ice should be given internally. July 15th. Continues to improve; the ice-bag was applied once this morning. Finding the head hot, I ordered warmth to the cervical region, the use of the ice-bag being discontinued. This patient has steadily recovered.

CASE XII.—Emily Eliza Gregory, age 7, sister of the above (under care of Mr. Bencraft, treated by me), was attacked July 16th at 2 A.M. with vomiting, purging, and cramps; became collapsed and quite pulseless. Ice was applied for the first time at 10 A.M., and was continued uninterruptedly until 4 P.M., when the countenance had become much less choleraic, and the vomiting, purging, and cramps were already greatly subdued. I then recommended the application of ice in the lowest cell of the ice-bag, and in the middle one up to the top. Warmth was simultaneously applied to the extremities in this as well as in the preceding case. From this date the child continued to improve, and is doing well.

CASE XIII.—Mrs. Witt, age 63 (under care of Mr. Bencraft, and treated by me). After suffering from diarrhoea for some days, while assisting to nurse the children just named, she was attacked with cholera. She entered the room when I was there, suddenly sank down, uttered an exclamation, became deathly pale, strikingly cold, the lips turning livid, and the sweat exuding in large drops over her face and upper extremities. I found her quite pulseless, her head being cold. She was carried to her own house, when it appeared there had been a discharge from her bowels as she sat. I applied ice immediately along the whole spine, and in about five minutes afterwards her pulse became distinctly perceptible, slight colour returned to her face, and in a few minutes more she said, "I am better." Ice to the spine and heat to the extremities were continued at intervals for some days. She steadily improved, and was out of danger when I left Southampton. On several occasions, when for want of ice in the house none was applied to the spine, vomiting and purging returned, to be again subdued when the ice-bag was re-applied.

*Cases 14 to 21 were under the care of Dr. Griffin.*

CASE XIV.—Mary Goodwin, age 19, was attacked July 12th; became collapsed and nearly pulseless. The attendant of this patient

having procured ice, but having no ice-bag, placed lumps of ice in a row, supported on each side by saw-dust, and then caused the patient so to lie upon it that the ice came in contact with her along her spine. An ice-bag was subsequently applied two or three days. The patient completely recovered.

CASE XV.—Charles Adams, age 7, attacked July 12th; became completely collapsed; had the spinal ice-bag applied “off and on” during two days, and quite recovered.

CASE XVI.—Mrs. Peters, age 24, was attacked July 15th. Had all the symptoms of cholera. The ice-bag was applied at intervals during many days. She was quite rescued from collapse, had no consecutive fever, but miscarried. She was slowly recovering when I left Southampton.

CASE XVII.—A girl, named Feltham, age 6, was attacked July 19th. She became collapsed, in a slight degree however, and continued to have a slight pulse. Under the use of the spine-bag she was recovering when I left Southampton.

CASE XVIII.—Mr. Willis, age 56, was attacked July 18th; became collapsed, his pulse, however, continuing perceptible. He was treated by means of the spinal ice-bag, and was recovering when I left Southampton.

CASE XIX.—Penny, a girl, age about 12, attacked July 18, became collapsed, but continued to have a slight pulse; she was first treated by Dr. Griffin, who applied ice to her spine, and subsequently she came under my care. She was recovering when I left Southampton.

CASE XX.—A patient of the name of Filder, of whose age or sex I have no note, was attacked July 19, in much the same manner as the patient last mentioned, was treated in the same way, and was also recovering when I left Southampton.

CASE XXI.—George Thorn, age 11, was attacked July 15th with very profuse vomiting and purging (rice-water stools), and violent cramps. Ice was applied to his spine during the whole day of July 16th, and at night all active symptoms had subsided. On the 19th he was up again and at play.

CASE XXII.—(Under the care of Dr. Olliver, treated by Dr. Chapman) Mrs. E. Allan, age about 50, has suffered during six or seven days from diarrhoea and vomiting, and many times from cramp; was admitted into Anspach House in a state of collapse, July 19th, 5.30 P.M.: was vomiting constantly, very cold, pulse just perceptible. I applied ice at once along the whole spine, and heat to the lower extremities: no heat was applied to the upper extremities, there

being no suitable means of doing so at hand. In the course of a few hours her skin improved, she became warmer, especially in the lower half of the body, her pulse became stronger, and, when I last saw her, in the morning of July 21st, the purging and vomiting, according to the statement of the nurse, had subsided. Ice was applied to this patient, from 5.30 p.m. of the 19th until about 7 p.m. of the 20th.\*

CASE XXIII.—(Under the care of Dr. Olliver.) A girl, named Maguire, about 10 years old, had been suffering for some days from cholera, she was treated successively by astringents, calomel, and arsenic, each of which failed to arrest the vomiting. She was therefore placed under my care. I treated her by means of ice along the spine, by which the vomiting was speedily and completely subdued. The girl was steadily regaining her appetite and strength when I left Southampton.

CASE XXIV.—(Under the care of Dr. Cheeseman.) Eliza Randall, attacked July 16th with cramps, vomiting, purging, and partial collapse. Dr. Cheeseman gave her a grain of calomel every half hour, and ice internally: the cramps were relieved, and the purging lessened, but the vomiting persisted. She was then treated by dilute sulphuric acid, which was also powerless to arrest the vomiting. She was therefore sent to Anspach House and placed under my care; in the evening of July 17th one bag of ice was applied along her spine, she vomited once, but expressed herself as immensely relieved and comforted by the application. She begged to have another bag during the night, which, however, the nurse did not supply. She rallied rapidly, notwithstanding, and ascribed the beginning of her improvement to the use of the ice. The next morning Dr. Cheeseman found her much better, and then saying she was scarcely a fair case to test the efficacy of the ice, resumed her treatment by means of stimulant salines.

The above are all the cases which have been treated successfully by means of ice at Southampton during the present epidemic. The following are all of those which have been treated unsuccessfully by means of ice.

*The first 8 cases of this list were under the care of Mr. Bencraft.*

CASE XXV.—Julia Ann Hall, age 5, attacked July 14th, had ice applied only thirty minutes before her death. In this case, as Mr. Bencraft observes, the child was moribund when the ice was applied, and in the short space of half an hour the ice had no chance of exciting its remedial power.

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\* I have been informed that after I left Southampton ice was no longer applied to this patient, that she was then treated by hypodermic injections, and died shortly afterwards.

**CASE XXVI.**—Caroline Powell, age 37, attacked July 13th very early in the morning, and died July 14th about 5 P.M.: ice was applied to the spine about 10 A.M. July 13th, and the application was continued until about 3.30 P.M. the next day. Mr. Bencraft says he was careful to have the ice properly applied, and is satisfied that it was so; Dr. Wiblin, however, informed me that he saw the patient, and found that the ice had slipped away from the spine. In the course of my directions for the treatment of cholera, published last year, I said, "The general principles of which the treatment here proposed is an application—dictate, moreover, certain auxiliary remedies which ought to be had recourse to in all severe cases. 1. The operation of the spinal ice-bag in overcoming the vasic-spasms generally should be facilitated by keeping the patient thoroughly warm in bed by means of an ample supply of blankets. 2. Co-operation of the like kind should be afforded to overcome the special coldness (vasic-spasms) of the abdomen by the application of heat over its surface. 3. Co-operation of the like kind and in like manner, by local applications, should be afforded, in order to overcome the special coldness (again vasic-spasms) of the hands and feet."\* Now there can be no doubt the case of Mrs. Powell was a severe one, but these "auxiliary remedies" were resorted to in no respect whatever, although her extremities were livid, as I can testify, to the extreme degree of seeming as if they had been black-leaded. Nevertheless, according to the statements of Mr. Bencraft, Dr. Wiblin, and Dr. Mac Cormack, the patient rallied considerably during the first five hours of the application of the ice. She became warmer, and meanwhile her cramps, purging, and vomiting completely subsided, and did not recur. I do not hesitate to express my belief that if all my published directions had been complied with, this woman's life might have been saved.

**CASE XXVII.**—Una Sophia Gregory, age 10, attacked July 13th, at 3.30 A.M. Ice was applied at 1.30 P.M. continuously until the evening of the 14th, when she was seen by me for the first time. Reaction had then become established, conjoined with undue determination of blood to the head, which was hot. I advised discontinuance of the ice to the spine, the application of a small piece of muslin dipped in iced-water to the forehead, and such nourishment as could be taken. The cerebral symptoms continuing, I advised the application of heat (water at 115° in a spine-bag) to the upper third of the spine. I was most emphatic and circumstantial in cautioning the child's mother not to let the water used be too hot; to observe carefully the temperature of the forehead, and to remove the bag immediately she should find that the forehead began to become cool. Unfortunately, as she told me on the occasion of my next visit, the head became stone cold and the back very hot, when soon afterwards the child sank. It seems to me likely that in this case if the heat

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\* See "Diarrhœa and Cholera." First edition.—p. 26.



of the water used had been regulated by a thermometer, and if the water had been applied judiciously, and had been withdrawn at the right moment, the child would have recovered.

CASE XXVIII.—Shelly, a boy, age 13, attacked July 13th, was also completely recovered from collapse, but lingered six days, suffering from cerebral disorder. Shortly after recovering from collapse he passed a large round worm. It seems to me not improbable that the presence of worms in his intestines may both have predisposed him to suffer from cholera, and may have induced the persistence of cerebral complications which, in his weakened state, would not yield to treatment.

CASE XXIX.—Martha Kerby, age 24, whose child died of cholera, and was buried July 14th, was attacked the same night. I saw her for the first time at 10.30 A.M. July 15th, when she was completely collapsed and pulseless. Had continuous cramps and the usual evacuations from the stomach and bowels. She presented the choleraic countenance in a striking degree; the face, legs, and arms were markedly livid; the calves of the legs were very tender on being touched; the skin generally was cold and bedewed with the usual clammy sweat; and she was extremely restless and anxious. I began to treat her at once. At 12 A.M., or within less than an hour and a half, the skin became drier, its lividity had lessened, the feet had become warmer; she had had no movement of the bowels, and had vomited but once. Pulse still imperceptible. The ice-bag being still cold I left her, and promised to return in half an hour. 2 P.M. Being detained by other patients I could not return to her till now. The ice-bag had become warm, and her vomiting, diarrhoea, and cramps consequently returned. I at once applied ice again, enjoined the application of heat to the limbs, and left her. 5 P.M.—Has vomited two or three times; the bowels have been opened once (rice-water stool), and the cramps still continue slightly; the head is warm, upper extremities moderately cold; lower extremities warm down to the ankles, feet cold; she is on the whole decidedly better than at two o'clock, but she is still pulseless. I ordered the ice to be applied in the lower cell of the ice-bag, and in the middle one filled to the top. Iced water and iced beef-tea *ad libitum*, warm water-bags to extremities.

11.30 P.M. Cramps, vomiting, and purging have ceased, and she has kept a cup of tea as well as beef-tea; but she is pulseless, or, as Mr. Bencraft thinks, has a slight flickering pulse; treatment to be continued.

July 16th, 10 A.M. Has had no ice on since 1 or 2 A.M.; and all bad symptoms have returned, the skin having become clammy and colder, with cramps, vomiting, and purging as before. Ice to be re-applied as soon as obtainable. 1 P.M. Is again much better, and has retained tea and beef-tea, as she did last night, under the influence of the ice.

*Comment.*—The foregoing notes were written immediately after each visit; and the facts they record prove beyond the possibility of a doubt that in this case the disease, which was of an extremely severe type, was amenable in a wonderful degree to the treatment adopted; although it was very imperfectly carried out. I believe I am correct in saying that on every occasion when I called, except at 12 A.M., July 15th, I had to deplore gross neglect in some form: generally the ice in the bag was melted, and the bag had become warm; at one time there was no ice in the house; at another the bag was on one side instead of along the spine; and always the heat ordered to the extremities was applied only partially, or not at all.

The woman lay in a small crowded room on dirty things, her clothes and bed being saturated with her evacuations. In the afternoon of July 16th a house for the reception of cholera patients had been prepared, and my hope of saving her was much strengthened by the expectation of removing her thither. I was disappointed: her mother, partially blind and paralytic and powerless to help her, would not consent to her removal. Discouraged by this woman's obstinate folly, I felt it useless to work against such odds; and from that time neglected the case, of which I took no further notes. In looking back upon it, I am sorry I did so; for the facts I have recorded justify my belief that even under the adverse circumstances mentioned I could have saved her had I persevered.

CASE XXX.—Caroline Seaman, age 38, attacked July 16th, after extreme exhaustion, consequent on nursing her son Shelly, named above. I only saw her once after her attack; it was impossible to treat her properly, and she was ordered to be removed to Anspach House, but died just as the men arrived for her.

CASE XXXI.—W. G. Tucker, age 9, attacked July 16th. A spine-bag and ice were supplied; but the mother of the patient told me she could not keep it along the spine.

CASE XXXII.—Emily Leake, age 22, attacked July 19th, and after being in profound collapse for some hours, was removed to Anspach House. Ice was applied along the spine, and unfortunately two accidents occurred in succession: owing to the imperfect closure and leakage of the bag, two night-dresses, one after the other, were drenched with iced water, thus adding to her coldness, and making it necessary to add to her exhaustion by moving her twice to put a dry dress on. The ice having been ultimately applied properly, and heat having under my superintendence been carefully applied to the extremities, the patient rallied. I left her decidedly improved, and having enjoined careful attention to keep the heat well applied to the extremities, was full of hope of saving her. But on returning to her in about two hours, I found her limbs uncovered, no heat in contact with them, and the collapse more profound than ever. She died shortly afterwards.

*The following patients were under the care of Dr. Griffin.*

CASE XXXIII.—Mrs. Macey, age 67, attacked July 13th: ice was applied, but not kept along the spine, I found it on one side; and no heat was applied to the extremities. She died on the second day of treatment.

CASE XXXIV.—Mrs. Bishop, age 32, attacked July 17th. This was the most severe case of cholera I have seen. She was seen two hours after seizure with diarrhoea, and was regarded as a light case. Two hours afterwards she was in profound collapse, and in about two hours more ice was applied. Heat was partially applied to the extremities, and even in this case some slight reaction was obtained during three or four hours. She died the next day.

CASE XXXV.—Nathaniel Vine, age 63, attacked July 16th. The first time the spine-bag was applied, the mouth of it was not properly fastened, and as quickly as the ice melted the shirt and bedding of the patient were drenched with iced-water. Only one bag of ice was afterwards used, and heat was only partially applied. He died the same night.

CASE XXXVI.—Margaret Saxby, age 8, attacked July 18th (in the same house in which the previous case occurred), was drenched with iced-water in the same manner; was allowed to lie in the cold wet clothes, and had no heat applied. Of course she died also.

So far as I am aware, the only medical men in Southampton who, during the present cholera epidemic, have had any patients treated by means of modifying the temperature of the spinal region, are Mr. Bencraft, and Drs. Griffin, Cheeseman, and Olliver.

The aggregate number of cholera patients treated by them up to the 20th of July was 72: of these, 44 were submitted to ordinary treatment; 25 had died before I left Southampton on the 21st instant, and of the 19 survivors, some had recovered, and some were recovering. Of the 28 who had been treated by means of ice, 13 had died before the 13th inst.; and of the 15 survivors, some had recovered and some were recovering.

According to these statements, it appears that whereas only 43 per cent. of those patients submitted to ordinary treatment have recovered, 53 per cent. of those treated by means of ice have been saved. But it will be observed that in Case XXV. the child was moribund when the ice was applied; that in Case XXXI. the ice was not kept along the spine; and that in Cases XXXV. and XXXVI., owing to the ignorance or stupidity of the attendants, the patients were drenched with iced-water, which escaped from the unfastened bags. These four cases therefore cannot be held to

have been cases in which my method of treatment was tried at all. If these be excluded, then of those treated 60·2 per cent. had recovered, or were recovering, when I left Southampton.

But this comparison of the aggregate results of the two methods of treatment, is far from showing the full remedial power of my method of treating cholera, as compared with the others adopted in Southampton. Of the whole 23 patients treated by Mr. Bencraft, the only four who survive were treated by ice. Again, if Dr. Griffin's patients be ranged into three classes of cases, viz., Cholerine, Slight Collapse, and Complete Collapse, as he has carefully distinguished them, it will be found that there are 10 of the first, 5 of the second, and 7 of the third class. Now, as all the patients treated by him, or in conjunction with me, by means of ice, were of the second and third class only, it is necessary, in order to institute a correct comparison of the two methods, to ignore the 10 cases of cholerine, and to compare the results of treatment in respect only to the cases of slight and complete collapse. The results are as follows:—

*Cases under ordinary treatment.*

Slight Collapse 5, all recovered.

Complete Collapse 7, all dead.

*Cases under Ice treatment.*

Slight Collapse 3, all recovered.

Complete Collapse 10, of whom 5 recovered.

It thus appears that while *all* the cases of complete collapse, viz., 14, which were submitted to ordinary treatment by Mr. Bencraft and Dr. Griffin, *died*; 4 of Mr. Bencraft's cases, and 5, or the half of Dr. Griffin's cases, altogether 9 out of 22, or 40 per cent. of those treated by means of ice, recovered.

The results would have been much more favourable still if all the cases treated had been treated properly. For example, in Case XXVII., death was most probably induced by the improper use of the hot water-bag; Case XXX. was only seen once, and can scarcely be said to have been treated at all. Moreover, whereas an essential part of my method of treatment consists in applying heat thoroughly and constantly to the general surface of the body in all severe cases, while the ice-bag is being applied along the spine, in Cases XXVI. and XXXIII. this was not done at all, and in Cases XXIX., XXX., XXXII., XXXIV., it was done but very partially and inadequately. Nevertheless, besides the two fatal cases in which complete reaction from collapse was obtained, in cases XXVI. and XXIX. the improvement effected was so considerable as to assure me that they would have recovered had they been in all respects properly treated; and even in Cases XXXII. and XXXIV., so much reaction was obtained as to forbid despair while there is life, if only the treatment in question be thoroughly and energetically persisted in.

Every one who knows how powerless medicine is to rescue patients from collapse, must admit that the results here described deserve the earnest attention of all interested (and who is not?) in finding a successful remedy for cholera; for it appears that out of the 24 cases treated, of which 22 were in collapse, 15 were saved, 2 were completely recovered from collapse, 2 were rallied to a great extent, and 2 in a lesser degree, notwithstanding the partial and ineffectual way in which the treatment was tried.

In presence of these facts—verifiable by authentic documents in my possession written by me from the dictation of the several medical men to whose patients they refer—the question immediately arises, How comes it that my method of treating cholera is reported to have been abandoned at Southampton? The reason is as follows: Mr. Bencraft and Dr. Griffin were known to be treating cholera patients by means of ice, and Dr. MacCormack, whose duty it is to keep a record of all the cases of cholera at Southampton, assumed that every case of cholera occurring in the practice of those gentlemen was treated by my method, and therefore alleged that every case of death from cholera which they reported was a case in which my method of treatment had been adopted. As a matter of fact, up to the 20th of July, Mr. Bencraft had reported 23 cases of cholera; of these 11 were not treated by ice at all, and they all died. And up to the same date Dr. Griffin had reported 35 cases; of these, 22 cases were not treated by ice at all. As already stated, 10 of these were cases of cholera, 5 were cases of slight collapse, and 7 of complete collapse; those of the two first groups recovered, all of the last died. So that, in fact, 11 of Mr. Bencraft's and 7 of Dr. Griffin's fatal cases are officially reported by Dr. MacCormack to have been treated by means of the spinal ice-bag; whereas in no one of these cases had it been applied at all! According to him, 31 deaths followed the treatment of cholera by ice in Southampton; whereas, in truth, only 13 deaths followed that treatment.

I observe that in the communication of the Southampton correspondent to the *Lancet*, and to the *Medical Times* of August 4th, 1866, it is stated: "The ice-bag applications to the spine are now considered as a thing of the past; indeed, unprejudiced observers think them worse than useless." It is of course competent for "unprejudiced observers" to "think" as seems to them best; but so long as they fail to impugn the validity of the above statistics, their opinion that "ice-bag applications to the spine are a thing of the past" will not be likely to exercise any influence on the final verdict which will be pronounced concerning them. As already shown, if the whole 28 cases nominally treated by ice be considered, the proportion of recoveries is 53 per cent. But if the four which were really not treated be excluded, then the proportion of recoveries is 60 per cent. (or 2 per cent. less than that of the recoveries by means of ice at Southampton, in 1865), under all the disadvantages of inadequate trial already described. Though at the time when I last left

Southampton, the proportion of recoveries under ordinary treatment was, as already stated, 43 per cent., the results as published in the *Medical Times* of August 4th, 1866, show a still less proportion of recoveries under that treatment. Up to July the 31st, there had been 158 cases, and 101 deaths: now, after the 28 cases treated by ice, 13 of which were fatal, are deducted from these numbers, it appears that 130 cases have been submitted to ordinary treatment, and that 88 of them have proved fatal, thus leaving 42 patients, or 30 per cent. who had recovered or were recovering. Concisely stated, the results of my method of treatment, as compared with the others pursued at Southampton, are as follows:—*nearly two-thirds of the patients treated by ice recovered; more than two-thirds of the patients submitted to ordinary treatment died.*

It is a not unreasonable presumption that inasmuch as those medical men at Southampton who applied ice to the spine were not using a remedy originated by themselves, their judgment of its effects would be neither perverted nor obscured by enthusiasm; while their actual experience of those effects would entitle their judgment of the merits of the remedy to more respect than that of any "unprejudiced observers" who had not equal opportunities of observing its action. Now the cases treated by ice at Southampton in 1866 were under the care of Mr. Bencraft and Dr. Griffin. Mr. Bencraft expressed to me his conclusion as follows:—"If I were attacked with cholera I should still wish to be treated by ice; but I should like to have it applied earlier than seemed to me necessary last year:" and Dr. Griffin's last words to me were—"Well, I don't see why such a dead set is made against the ice: it stops the cramps, vomiting, and purging; it makes the patients warm, and it prolongs life."

In the *Lancet* of August 18th, 1866, there is a report by Dr. J. Wilson McCloy, of his treatment at Liverpool of seven cases of cholera, by what I presume he will call my method. He says—"Ice to the spine, either alone or alternated with hot-water bags, was miserably unsuccessful. The application did not seem to have the slightest effect in producing reaction where there was any considerable collapse. While the ice-bags to the spine were borne without complaining, a similar application of water, at 120° Fahr., caused the greatest pain." Ice-water was given internally. Nothing is said of the application of heat to the general surface of the body, and I presume none was applied. All the seven patients died. They bore the spinal ice-bags, it is said, "without complaining;" but "a similar application of water, at 120° Fahr., caused the greatest pain." These facts are surely instructive illustrations of the therapeutical doctrine I advocate; and I certainly thought I had expressed that doctrine too clearly to permit of its misapprehension. The application of heat along the spine causes the arteries of all parts of the body, except in that *immediately* acted upon, to contract; therefore if heat be applied along the spine in any stage

of cholera, except that of reaction, it must inevitably aggravate the symptoms and sufferings of the patient: the circulation is still further retarded; the temperature falls still lower; the pulmonary structures are still more energetically collapsed, and thus increase the suffocative oppression with which he was already struggling; and meanwhile the glands of the skin and mucous membranes receive a fresh impulse to continue pouring out their secretions in excessive abundance. The dangerousness and probability of a fatal issue of such treatment is obvious and great; it can be no matter of surprise, therefore, that it should cause "the greatest pain," even when the temperature of the water used is only 120° Fahr. It will now be readily understood that a treatment consisting in the alternate application of cold and heat along the spine must be worse than useless; that Dr. McCloy's application of spine-bags in the seven cases of cholera in question was in no respect a trial of my method of treating the disease, and that the fate of the patients, in so far as it was influenced by that application, is only another testimony to the truth of the doctrines explained and exemplified in this work.

An American gentleman informed me a few days ago that during the present epidemic ice has been applied in bladders along the spine of patients in New York and Brooklyn, and that the results were reported to be very satisfactory. My informant is not a physician, had not seen any cases, and could furnish me with no particulars. The great, indeed, world-wide importance of the question—Is the foregoing exposition of the nature and treatment of cholera verified by experience? renders it extremely desirable that a large number of authentic facts, bearing on this question, should be collected and published as soon as possible. I shall therefore be glad to receive from any quarter reports—the more circumstantial the better—of any cases of diarrhoea and cholera which have been treated by the method here proposed.

A circumstance very remarkable, both as an empirical anticipation of my discovery, and as an evidence of its value, was related by Professor McLean in the course of his remarks at the close of my Lecture on Cholera at Southampton, already referred to. He said—"He was conversing with Dr. Davidson, one of the assistant professors of hygiene at Netley, and he mentioned a circumstance in connexion with Dr. Chapman's treatment. A few years ago Dr. Davidson came down the Ganges with a considerable detachment of troops and a large number of passengers. They were prostrated by extreme sickness, and almost all suffering from a virulent form of diarrhoea, though not apparently of a choleraic description. Meeting another steamer, they signalled to her, and a medical man who happened to be on board came to them. Immediately he came he ordered a discontinuance of the treatment in the use of drugs, and having a supply of ice on board, he used it in much the same way described by Dr. Chapman. Dr. Davidson assured him that the effect was most signal, and that a large portion of the cases imme-



diately recovered, among them being the captain, who was so prostrated as scarcely to be fit to carry on his duties.”\*

While closing these pages, I have had another opportunity of proving how invaluable is the remedial power of ice in bowel affections. The following are the facts of the case:—

CASE XXXVII.—Mrs. P. was attacked in the night of September 22, 1866. During the previous morning she passed an extraordinarily large quantity of pale urine. After experiencing several alternations of heat and cold she became very cold; and her skin was suddenly contracted so as to leave its papillæ in extreme relief, imparting to it the peculiar aspect well characterized as “goose-skin.” The abdomen was especially cold and very hard. The arms became rigid, and attempts to move the fingers caused “very great pain” in the flexor muscles. The head was particularly cold. There was a most distressing sense of suffocation; and a feeling—especially when she stood up—as if she was going to faint, accompanied with a loud noise in the ears like the roaring of the sea. She had also severe abdominal cramps, which seemed to extend to the womb; convulsive twitches or jerks in various parts of the body; and forcible flexure of the toes, which she could not move by an effort of the will. Vomiting and purging, both extremely profuse, came on very early in the morning, and recurred at very frequent intervals. I saw her soon after daybreak: her face was then of a dark or brown colour, resembling that of oak or bronze; she was extremely cold, restless, anxious, faint, and exhausted, her pulse being very feeble, though quite distinct. Her bowels had already been moved seven or eight times, and the vomiting was almost incessant. The stools retained their fæcal character, and the ejections from the stomach consisted chiefly of bile in extraordinary abundance.

She informed me, with some surprise, that she had not passed any water since the previous morning. I ordered ice to be procured at once; but during the time (about an hour) which elapsed before it was obtained she grew very much worse, reeled and needed support when she got up each time her bowels were moved; and the vomiting and purging, which were simultaneous, became more frequent.

As soon as the ice was brought I laid her upon a 22-inch spinal ice-bag; applied heat to the feet; covered her with several blankets as she lay on a feather bed; and gave her *hot weak* tea, as much as she would drink from time to time. She was calmed and comforted and the discharges were restrained with astonishing rapidity. Within five minutes she became sleepy, and within half an hour she was in a sound, refreshing sleep as she lay on the ice. During the forenoon she vomited and was purged two or three times, but the vomiting

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\* *Hampshire Independent*, October 7, 1866.

was slight and without distress, and, as she said, her feelings when the bowels were moved were more natural; but what was much more striking, she found when she got up the first time after lying on the ice, that not only had the suffocative oppression and sense of faintness ceased, but that she had suddenly recovered her strength, so that she could again walk with her usually elastic step. The skin quickly relaxed and resumed its wonted smoothness; she became quite warm meanwhile; and at the end of about an hour all abnormal phenomena of the muscular system had vanished. After the first hour ice was applied at intervals during the morning for about fifteen minutes at a time. By mid-day vomiting and purging had quite ceased; at 2 P.M. she took some food, and enjoyed it; at 4 P.M. she passed a very small quantity of water, for the first time since the morning of the day before, and then felt a further and specially notable improvement in herself in all respects. At the same time her countenance had already become lighter and of more natural hue.

Ice was applied along the lower half of the spine twice more before 11 P.M., during about thirty minutes each time; meanwhile she was progressing satisfactorily. But about five o'clock the next morning she found herself again distressed, much in the same way as she had been the previous morning before the vomiting and purging supervened. She felt the threat of a fresh attack most especially in the same oppression at the chest and difficulty of breathing which she had experienced before. Fortunately, as I had recommended, an ice-bag properly filled with ice and folded in a thick blanket to prevent the ice from melting had been provided and kept in the room; this was applied about half-past 5, and very soon it put an end to every unpleasant feeling. The patient afterwards slept, and then on rising felt as well as usual, except that she had been weakened by the attack. Nothing more was done, and she has continued wholly free of any vestige of the malady since that date.

*Comment.*—The effects of opium, in even very small doses, on the brain of this patient are so extremely distressing and injurious, that she absolutely refuses to take it; and the predominance of spasmodic symptoms would have made me fear to prescribe either it or sulphuric acid, lest by so doing I should have intensified the disease. In this position I should have despaired utterly of being able to combat it successfully by means of medicines; and its character was such that, had it not been arrested, it was more likely than not to have developed into choleraic collapse. Though the malady was stopped at an early stage, it presented several features proving its affinity with true cholera, viz., disorders of both the voluntary and involuntary muscular system; faintness, tinnitus aurium, dyspnoea, coldness, and discoloration of the skin, and suppression of urine. When once physicians realize the full extent of the remedial power which may be exerted by means of cold and heat applied along the

spine, they will enter on the treatment of difficult cases, which would otherwise baffle their resources and fill them with despondency, with a confidence in the agents at their command, and a consequent hope of success which will add immensely to the pleasure of practising medicine, and will banish that modern innovation—do nothingism, which has grown out of the recognition that in many cases to give medicines is to run risks of doing harm, and which is known by the high sounding title, “The Expectant Treatment.”

Probably I shall be asked the following pertinent question:—“Holding the doctrine you do concerning the nature of cholera, and possessing the amount of evidence described in this chapter of the efficacy of your method of treating it, how is it that during the recent epidemic invasion, which medicine is still confessedly impotent to resist, you have neither exemplified the value of your method nor said a word in the public journals concerning it?” By anticipation I will answer this question as simply and briefly as I can. *First*, I have had no private patients, beyond those already mentioned, who have suffered from cholera. *Second*, I have not been invited by the authorities of any hospital or public institution to make trial of my remedial plan. *Third*, I applied to the chief of the medical department of the Privy Council, Mr. Simon, to afford facilities for trying the treatment effectually in London: he rather encouraged me to hope he might; but though it is reported that a grant of money has been placed at his disposal by the government for the special purpose of instituting researches into the nature and treatment of cholera, I have not heard from him since, and fear that the pathology of cholera here expounded stands too far apart from the doctrine concerning it now most generally recognised in the medical world to have any chance of early examination and recognition by official authority. *Fourth*, I communicated with a wealthy person deeply interested in assuaging human suffering, and was allowed to expect that a small hospital for the reception of cholera patients to be treated by me would be established and placed under my control; but for reasons unknown to me the scheme fell through. I regret this extremely, for I believe that had such an opportunity of giving a practical exemplification of my views been accorded me, the twofold problem,—What is the nature of cholera? and, How can it be treated most successfully?—would have been so far solved, that a large number of those lives which are now being destroyed by the disease in different parts of the world might even this year have been saved. Indeed, so confident am I of the truth of the doctrines set forth in this volume, and of the unspeakable importance of their practical consequences, that had I adequate private resources I should establish at my own expense a hospital in which I might demonstrate at once the principles and practice of

what I call neuro-therapeutics by examples and results which, in my opinion, could not fail to convince even the most sceptical observers. *Fifth*, it seemed to me of paramount importance to hasten the publication of this work; and I had to choose between doing so and expending the time needful for its completion in seeking opportunities of gaining more practical experience of the merits of the method in question by the treatment of additional cases. I decided that it would be most expedient to prepare the foregoing expression of my views as speedily as possible, in order that the medical profession in different countries may forthwith possess the requisite data for judgment of my hypothesis and full explanatory directions how fairly to try it by the test of experiment. I confess, however, that I hoped to have completed the work—so far at least as it is now complete—some weeks ago, and therefore while the present epidemic was still at its height; but though disappointed in this respect, I shall not be surprised if the cholera lingers in England sufficiently long this year to afford many opportunities yet of proving the accuracy of my statements before the disease finally disappears.

I strongly recommend those physicians who are disposed to avail themselves of the remedial power of cold and heat applied along the spine in the treatment of bowel affections to make a beginning with cases of diarrhoea and cholérine. As these phases of cholera are much less formidable than actual collapse, they can be treated more at leisure and with less anxiety as to the result than is possible in cases of collapse; hence more opportunity is afforded for calmly observing and studying the effects of the treatment; and as the patients in these early stages are less restless and exert more control over themselves, they are more likely, without the aid of an ice-bag jacket, to keep the ice exactly along the centre of the spine, and thus to give it the best chance of exerting its utmost beneficial influence. Moreover, in the majority of such cases the need for the application of heat to the general surface of the body while ice is being applied to the spine is much less urgent, as hot-water bottles to the feet only will often suffice. Therefore in these cases the effects produced can be almost exclusively ascribed to the spinal ice-bag as their cause, and the most exact conceptions can be formed of its therapeutical value. Such patients have not yet become seriously anxious or alarmed about themselves, and retain nearly all their mental vigour and self-possession; their power of observation and description being comparatively unimpaired, they can give an exact account of their symptoms before anything is done, and of their experience from hour to hour of the effects of the ice on each segment of the body after the treatment is begun, and while it is continued. In this way it may be accurately studied, and he who thus really acquaints himself with it will find himself most fitly prepared to treat the

disease in its most formidable aspect of collapse. The wise precept, "Proceed from the simple to the complex," or the more homely saying, "First walk, and then run," is just as applicable here as it is in every part of life; and I may observe here, that the determination to test the efficacy of the treatment in question in the worst cases first has not been most conducive to a speedy as well as correct appreciation of its value. The number of fatal cases of diarrhœa in London during the passing summer has been very large, and the returns of each week still show that about a third of all the persons who are destroyed by diarrhœa and cholera conjointly, die of diarrhœa, notwithstanding the asserted power of medicine to subdue this disease. Surely if, as I assert, the spinal ice-bag is a greater power, it would be worth while to try if it would not save these lives. But indeed diarrhœa does not seem to arrest the attention and excite the interest of even the medical profession to an extent proportionate to its destructiveness. The weekly average mortality of London may be increased by the death of a hundred persons by diarrhœa without causing any interest or surprise; but if it be increased by only fifty deaths from cholera, serious apprehension is felt, and the public journals resound with alarm. There may be, and I dare say there is, adequate reason for this; but that diarrhœa is, comparatively, a neglected disease, and that its great dangerousness and fatality are not duly borne in mind are facts which can scarcely admit of dispute.

I do not hesitate to affirm that the evidence and arguments contained in this volume justify the conclusion, that by the intelligent and judicious use of the spinal ice-bag death may be averted in every case of infantile diarrhœa, summer diarrhœa, cholera, and the early stages of so-called Asiatic cholera; and that were each soldier in India compelled to apply one bag of ice along the lower half of the spine until the ice has melted during those days when he is marching, we should hear no more of the extraordinary phenomenon of the especially great mortality of soldiers in India when moving from one encampment to another.

What per centage of recoveries in cases of actual collapse can be effected by the treatment recommended in the previous chapter remains to be proved; but the facts already adduced, showing that, notwithstanding the extremely adverse circumstances under which the patients at Southampton, both in 1865 and 1866, were treated, 26 out of 33 in collapse were completely rescued from that state, and four more were rallied, two of them to a great extent, have a favourable significance which it would be difficult to overstate: and when considered in conjunction with Dr. Griffin's assertion that the spinal ice-bag, when used alone even, stops cramps, purging, and vomiting, makes the patient warm, and prolongs life, encourage the most sanguine hope that when ice along the spine and heat over the general surface of the body are properly applied, and

when all my other injunctions are complied with, as Dr. Wiblin rightly insists that they must be, cholera will be ranked among those diseases over which medical science has obtained remedial control.

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NOTES.

1. *Prevention of Cholera.*—As the disease is now spreading in various parts of England I am induced to add a few words of advice concerning personal precautions for its prevention. I am entirely convinced that if those persons who happen to be within the area of the epidemic influence, were to wear the spinal ice-bag once daily, and at any times additionally when they feel any apprehension or symptoms of an attack, they might move about in the very foci of the pestilence unharmed. Many persons, the “nervous” especially, experience during cholera-times various anomalous symptoms, directly referrible to the nervous system; for example, slight twitches, jerks and spasms, or even considerable cramps in different parts of the body, without the supervention of diarrhœa; such persons are those most likely to be anxious or alarmed in the presence of epidemic cholera, and their fears predispose them to become its victims. In all such cases a very short experience in using the spinal ice-bag will dissipate their apprehensions, and so improve their general feelings as to assure them of immunity from the disease. Those who think the application of a spinal ice-bag a formidable affair, will think very differently after they have once used it, especially if they really need it.

2. *Post-mortem State of the Sympathetic System and the Pneumogastric Nerves in Cases of Death during Choleraic Collapse.*—In sections eight and nine, Chapter II., Part II., the evidence adduced concerning the post-mortem state of the nervous system is conflicting. More precise knowledge is now in a great measure, at least, supplied by Dr. George Steele in a paper entitled “Some Remarks on Cholera, with Reminiscences of the Epidemic of 1832,” and published in *The Edinburgh Medical Journal*, for September, 1866. He says, “I am anxious to call attention to one feature which was very constant in the cases I examined, and which, though it cannot be without importance, has apparently, either not existed or escaped the notice of writers since 1832. I allude to a singularly hyperæmic state of the sympathetic system and the pneumogastric nerves. It was not unusual to find congestion more or less extensive in every portion of the sympathetic chain of ganglia. The splanchnic nerves, the semilunar ganglia, and the solar plexus were the parts chiefly involved, and these, not unfrequently, were imbedded in extravasated blood. The cœliac, renal, œsophageal, cardiac, and pulmonic plexuses exhibited morbid lesions of the same kind but

generally to a less extent. The neurilemma of the pneumogastric was frequently injected, and studded with ecchymosed spots; and in one case I found the phrenic nerve, where it spreads out upon the diaphragm, steeped in a large coagulum. In one case, also, Mr. Lizars found the same nerve involved, its neurilemma in the thorax being injected all the way to the diaphragm" (p. 241).

When I first gave a brief expression to my views of the pathology of cholera in 1864, and again in 1865, I arrived, by reasoning only, at the conclusion, that the essence of the disease consists in hyperæmia of the sympathetic ganglia and of the spinal cord. The anatomical justification here supplied of the correctness of the doctrine which pervades this volume will go far to remove that doctrine from the region of mere hypothesis. and to rank it among established truths.

3. *Spine-Bags*.—These bags, so often mentioned in this volume, are of two kinds. One kind, used chiefly for the application of ice, is divided into two, or three, or more cells, either by external clamps or by internal septa, in order to hold the ice or iced-water in apposition with the whole or any part of the back, the patient being enabled to remain upright or to walk about meanwhile. The mouth of each of these bags is closed by a clamp. The other kind, used chiefly for the application of heat, but also of moderate degree of cold, consist of two parallel tubes, a little distance apart, and communicating at top and bottom, the mouth of the bag being closed by a screw. They are fairly represented by the following woodcuts:—



The bags can be obtained from Messrs. C. Macintosh & Co., the

\* The width of the spinal-water-bag, as here delineated, is too great in proportion to its length.



Manufacturers, 8, Cannon Street, West, and from the following agents:—  
*London*: S. Maw & Son, 11, Aldersgate Street; John G. Gould, 198, Oxford Street. *Glasgow*: Thomas Chapman, 56, Buchanan Street. *Paris*: Thomas Stubbs, 97, Avenue Champs Elysées; Louis Hamelin 34, Rue de Penthievre. *Brussels*: Perry & Co., Maison Follett. *Philadelphia*: J. Mardon Wilson, Jun., North-east Corner of Ninth and Sansom Streets.

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"This is an interesting monograph, by a man of original mind and considerable ability . . . . Dr. Chapman states that, by the application of cold to the middle of the spine, it is possible to increase the force and frequency of the heart's action, and by heat to diminish it. This is opposed to the dicta of Dr. Todd and other clinical teachers, but Dr. Chapman alleges it as a fact, and challenges experiment. Carrying on his experiments, he erects into a therapeutical system the application of cold and heat to the spine in various parts, and for different periods of time, and claims for this treatment a philosophical interpretation and wide practice. His *brochure* requires reading and testing clinically. It is original in its views."—*Lancet*, Feb. 20, 1864.

"Dr. Chapman expresses his belief that many maladies not usually held to depend on nervous agency—such as those in the title (diabetes and constipation)—really originate in that manner. . . . The subject deserves the serious attention of the profession, not only from the high character of its originator, but also from the amount of evidence which he has already brought to bear upon it. He, however, seems to be as yet only at the outset of his investigations, and promises more detailed information hereafter. In so doing he will confer a great boon on physiology and practical medicine."—*Medical Times and Gazette*, Jan. 23, 1864.

"Under the influence of these remedial measures, Dr. Chapman has succeeded in six cases, related in the *Medical Times*, in effecting a cure or a marked improvement in epilepsy. This is a result assuredly deserving of attention, and fresh researches may possibly fecundate this ingenious application of a discovery highly creditable to contemporary physiology."—*Journal of Practical Medicine and Surgery*, Oct. 1863.

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## SEA-SICKNESS:

ITS NATURE AND SUCCESSFUL TREATMENT THROUGH THE AGENCY  
OF THE NERVOUS SYSTEM BY MEANS OF ICE.

"Certainly, so far as the history of these voyages across the Channel goes, it is highly in favour of the author's ingenious recommendations. . . . We advise, both for practical and theoretical purposes, that the pages of this pamphlet be carefully perused."—*Lancet*, March 4, 1865.

"If Dr. Chapman has proposed to us a means of alleviating, with almost perfect certainty, that most distressing malady, sea-sickness, we are criminally neglectful if we refuse to employ it. . . . The case [in question] is as conclusive as a single case can be, in regard to the great practical value of his discovery. . . .

"The effects of its application were little short of miraculous. In three minutes the retching ceased and the spasms were calmed. In a quarter of an hour she [the patient] had fallen into a quiet sleep; and in half an hour her hands and feet were of natural warmth, and her face had regained its wonted colour. In two hours she awoke, greatly refreshed, and from that time did not miss a single meal."—*Report of "Case of Sea Sickness successfully treated by Ice to the Spine."* By B. LEE, M.D., in the *Philadelphia Medical and Surgical Reporter*.

The treatment was continued during the whole voyage—a rough one, lasting four days and a half—with complete success. Dr. Lee rightly observes,—“I can see no reason why the process may not be successfully extended over twelve days as well as four, with proper care and management.”

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The following are abridgments of a few of the reports of cases given in Dr. Chapman's pamphlet of the cure of Sea-sickness by means of the Spinal Ice-bag:—

EXPERIMENT I.—Mdlle. H., who had crossed the Channel several times previously, and of whom Dr. Chapman testifies that he never saw a person suffer more from Sea-sickness, left Dover for Calais, February 9th, 1864, and applied one of Dr. Chapman's Ice-bags during the passage. She afterwards wrote—"I think nearly every one was sick, and I, the usually most of all, not only not sick, but even well, feeling as if I were in a cradle being rocked by the gentle hands of a tender nurse. . . . We reached Calais, and I had not experienced the slightest nausea."

EXPERIMENT II.—Mrs. H., who usually suffers extremely when at sea from sickness and intense headache, and who on two occasions had a series of epileptic fits induced by the ship's motion, crossed from Folkestone to Boulogne, May 12th, 1864, one of Dr. Chapman's Ice-bags being applied to her back during the passage. She reached Boulogne without any sign of sickness. This lady made five passages subsequently, using ice each time, and was confirmed in her conviction of the great efficacy of Dr. Chapman's method of treating the malady, from which she is an extraordinary sufferer.

EXPERIMENT IX.—Mdlle. G., who has been six times at sea, and who, on each occasion, before using the Ice-bag, suffered much from sickness, crossed from Newhaven to Dieppe, May 27th, 1864. She applied an Ice-bag during the whole passage, and reached Dieppe without experiencing the least sickness.

EXPERIMENT X.—Mrs. G., while crossing from Newhaven to Dieppe, June 11th, 1864, was very ill, vomited bile frequently, and complained of intense headache. Dr. Chapman applied ice along her spine: her sickness and headache ceased entirely, and she reached Dieppe continuing well.

EXPERIMENT XI.—Miss C., while crossing in the same boat with Mrs. G., June 11th, 1864, was lying down very sick, and complained especially of an acute headache. Dr. Chapman placed an Ice-bag along her spine as she lay,

the bag being next her skin. In a few minutes her head became quite free of pain. While lying on the ice she fell asleep, and in about half an hour awoke quite well, continuing so until she reached Dieppe.

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Captain White, commander of one of the Newhaven and Dieppe boats, in which some of Dr. Chapman's experiments were tried, says in a letter addressed to him—"I have given it [Dr. Chapman's remedy] great attention ; and am of opinion that in ordinary weather it is a success. I had some difficulty in persuading passengers to try it, but those who did were benefited."

In the *Lancet* of December 3rd, 1864, is a letter headed "*Dr. Chapman's Remedy for Sea-sickness*," and signed, "S. M. Bradley, Surgeon, Cunard Service." He says :—

"I have tried this [remedy] in *severe cases where other remedies have failed* (chloroform, iced champagne, effervescing draughts, fresh air, &c. &c.), and have very generally found it do great good. I have applied it to young children, delicate women, and old people. In no case does it do harm ; but in the great majority of instances it soothes the nervous irritability which so commonly accompanies sea-sickness, induces sleep, and so enables the stomach to receive light food, and consequently relieves exhaustion. . . . I order it to be kept on a couple of hours ; though, as the patient sleeps, as is often the case, I never remove it until after waking."

In the *Shilling Magazine* for July, 1865, occurs the following extract from the private letter of a physician (dated June 3rd, 1865), giving an account of an experiment made while crossing the Atlantic :—

"I recommended a patient about to cross the Atlantic to try one of Dr. Chapman's Ice-bags for Sea-sickness. The result was most satisfactory. He was never sick when wearing the ice-bag. Once he went without it, and then, and then only, was he sick."



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# MEDICAL OPINIONS

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*From the "Lancet," Feb. 20, 1864.*

"Carrying on his experiments, Dr. Chapman erects into a therapeutical system the application of cold and heat to the spine, in various parts, and for different periods of time, and claims for this treatment a philosophical interpretation and wide practice. His brochure (on the 'Functional Diseases of Women') requires reading and testing clinically. It is original in its views."

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"Dr. Chapman expresses his belief that many maladies not usually held to depend on nervous agency really originate in that manner. . . . The subject deserves the serious attention of the profession, not only from the high character of the originator, but also from the amount of evidence which he has already brought to bear upon it."

*From the "Medical Times and Gazette," June 14, 1873.*

"Dr. Chapman aims at something far more than a clinical generalization. . . . He would tell us of the immediate agency by which each and all of these disorders (of the nervous system) is caused, and, knowing this, he would give us a key to the direct treatment of them all. He does not shrink from saying that here we have a key to disorderly actions of muscles voluntary and involuntary, to morbid actions of glands, to disorders of local nutrition, &c.; that his method is a method of commanding a vast number of very various maladies. Indeed, although no vital function depends primarily and in its simplest terms upon the nervous system, yet in man this system has gained so great a control over all actions in the economy, that to command this system is practically to command the whole body. Dr. Chapman's system is, therefore, something like a panacea, and he asks to have it regarded in that light, and to have it called the doctrine of 'neuro-dynamic medicine.' . . .

"Dr. Chapman himself, however, speaks not as a mere theorist. He publishes in adequate detail a very large number of important cases, which together make up a large mass of evidence in favour of his views—evidence which cannot be disregarded. . . . These numerous examples cannot be without great weight; many of them, indeed, are very striking. . . . It is evident that if Dr. Chapman establishes any great part of his thesis, he has made one of the most remarkable therapeutical discoveries in the history of the art."

*From the "Medical Press and Circular," June 5, 1867.*

"We are not accustomed to devote our leading columns to the advocacy of any therapeutical system, but we feel it only due to a most able physiologist to testify to the necessity of submitting his conclusions to the test of experience. . . . Reasoning on the facts that had been demonstrated by Claude Bernard and Brown-Séquard, that a division of a portion of the sympathetic increases the flow of blood to the parts to which its branches are distributed, while galvanism of the nerve decreases the amount of blood in the same parts, Dr. Chapman directed his inquiries to the possibility of finding remedial agents that would depress or excite the nervous centres. Such a depressant, he concluded, might be found in *cold*, while the opposite condition of *heat* should, *a priori*, act in a manner analogous to galvanism. He put his conclusions to the test by applying heat and cold to the spine, and so satisfied is he with the result, that he has not ceased to urge upon the profession his belief that, by varying the temperature of the nervous centres, we may, at will, diminish or increase the vital activity of those parts which derive their nerve-supply from the portions on which we experiment."

*From the "Medical Press and Circular," April 30, 1873.*

"It is, indeed, quite time that the remarkable generalization long since arrived at by Dr. Chapman, and now supported by the vast array of facts and arguments before us, should receive the thoughtful consideration of all those engaged in the treatment of disease. His theory not only explains many morbid phenomena, but suggests a rational mode of treating the diseases on which they depend. This mode has been applied in



hundreds of cases with such success that Dr. Chapman is entitled to ask his brethren either to try it in their own practice, or to state their reasons for not doing so, and we venture to assert that those who carefully read this volume (his work on Neuralgia), will hesitate before they measure swords with the author."

*From "The Doctor," April 1, 1873.*

"Dr. Chapman has expounded a theory that every practitioner ought carefully to weigh, and has enforced his doctrines with a mass of evidence that entitles him to a foremost place among those accurate clinical observers who are helping to advance scientific therapeutics."

*From Professor F. W. BENEKE, one of the Editors of the "Archiv des Vereins für wissenschaftliche Heilkunde."*

"It seems to me that Dr. Chapman has done an important service in calling attention afresh to the intimate connection between many morbid phenomena, and morbid conditions of the centres of the nervous system, and to the immediate dependence of the former on the latter—especially on morbid conditions of the ganglia of the sympathetic. . . . Should the efficacy of his therapeutical measures be verified, even to a small extent only, the knowledge of his general view will conduce to fruitful meditation, and even an only partial confirmation of his therapeutical experiences would be already an extraordinary gain to our power of healing."

In a letter addressed to Dr. Chapman, Professor Beneke writes:—"I think that much can and will be done by the principle of applying cold and heat to the spine, which you have introduced; as far as the heat is concerned, I am fully convinced of its extreme usefulness in certain cases;" and again he says:—"I take always the greatest interest in the very important mode of treatment of certain diseases, which we are owing to you."

*From a Paper read to the Surgical Society of Ireland, March 13, 1868, by Dr. J. H. BENSON, Physician to the City of Dublin Hospital.*

"This grand object—a new therapeutic agent in our hands by which to control disease—seems to have been accomplished in a very simple way by Dr. Chapman, and by the use of heat or cold to the spine at great number of diseases seem capable of being successfully treated. This power, moreover, extends not only to nervous diseases, universally so called, but also to many of those which, previous to his discovery, were never considered to have had any causal relation to a deranged state of the nervous centres. Such, for example, are bronchitis, diarrhoea, constipation, leucorrhœa, and the usual disorders of menstruation, some skin diseases, and many others.

"Whether this therapeutic agent, then, is a powerful one or not is to be determined by experiment; and I think that those who give it a fair trial will agree that it is, whatever be their opinion of the soundness of the theories which are involved respecting it. In several cases of disease I have used this agent, but generally in conjunction with others, and, therefore, though satisfied in my own mind of its influence for good in those cases, I did not report them, for in order that the remedial value of any given general mode of treatment should be fairly tested, it is obviously desirable that the treatment should be used alone. In two cases, however (one of paralysis and one of suppression of the menses), cold to the spine was used successfully, unassisted by any other agent whatever."

*From C. H. ROUTH, Esq., M.D., Consulting Physician for Diseases of Women to the North London Consumption Hospital, and Physician to the Samaritan Hospital.*

"Dr. Routh said [at a meeting of the Medical Society of London] he had been to some extent a pupil of Dr. Chapman, and had witnessed his treatment of certain cases at the Samaritan Hospital. The result was the production of certainty in his mind of the truth of Dr. Chapman's doctrine—viz., that the circulation and nutrition of remote parts, or of the periphery of the body, may be increased by the application of heat to the same region; also, that the functional activity of the spinal cord may be depressed or exalted in the same way. These facts, he repeated emphatically, are thoroughly established and indisputable. Being so, they opened up the prospect of a great therapeutical revolution."

*From a Letter to Dr. CHAPMAN by Dr. BRERETON of Sydney, New South Wales.*

"I have fully satisfied myself of the great value of your discovery—a discovery not of a few isolated facts, but of a principle of treatment, capable of most varied application, and, like all principles, likely to lead to further, and as yet unimagined, results."

*From a Letter to Dr. CHAPMAN by Dr. HAYLE of Rochdale.*

"I take this opportunity of thanking you for a very effective additional means of com-

bating disease. Your discovery opens up a wide field of speculation as to the primitive and secondary action of medicines."

*From a Letter to Dr. CHAPMAN by Dr. JOSEPH M. O'FERRALL, late Senior Physician of St. Vincent's Hospital, Dublin.*

After stating that he had found the spinal ice-bag "very efficient in many cases of painful spasmodic affections seeming to depend on irritation of some portion of the spinal cord," he observes: "The details of these cases are certainly calculated to support your views of the therapeutic effect of the agent in question."

[Each of the three immediately following extracts is from a letter written when Dr. Chapman was a candidate for a certain Professional appointment:—]

*From W. B. CARPENTER, Esq., M.D., F.R.S., late Registrar of the University of London; Author of "Principles of Physiology, General and Comparative," and "Principles of Human Physiology," &c., &c.*

"I regard Dr. Chapman's medical researches as of the highest physiological interest and therapeutic value."

*From R. H. GOOLDEN, Esq., M.D., Oxon., F.R.C.P., Physician to St. Thomas's Hospital*

"I take this opportunity of acknowledging the advantage I have derived, in hospital as well as in private practice, from facts in pathology and therapeutics which the profession owes to Dr. Chapman's labour and acumen, and the correctness of which he did me the honour to demonstrate practically in my wards at the hospital."

*From ERNST HART, Esq., M.R.C.S., Ophthalmic Surgeon and Lecturer, St. Mary's Hospital, and Editor of the "British Medical Journal."*

"Dr. Chapman's highly interesting researches on the application of cold and heat to the spine as a curative method, and on the physiological effects of those agents, are very valuable, and probably admit of greater development than they have yet attained, when the limits of the therapeutic application are clearly defined. I have myself, carrying out Dr. Chapman's plan of treatment, met with success in a very intractable form of disease, and the profession is indebted to him for a valuable contribution to the healing art."

*From W. H. SANDAM, Esq., M.R.C.S. Eng. (published in the "Medical Press and Circular"), December 16, 1868.*

In his report of a "Case of Melancholia treated by Ice to the Spine," Mr. Sandham says:—"After having read the report of three cases of delirium tremens of a severe character cured by ice to the spine, and having myself had satisfactory results from ice applications in a case of epilepsy, I determined on trying ice in the present case, as directed by Chapman, to whom be all the honour he so well deserves. My patient was in a low state, unconscious, his eyes fixed and vacant, frightening myself and his family, as I feared a fatal termination before morning." Mr. S. then gives the details of his treatment with the spinal ice-bag, and having stated that the patient "is very nearly mentally and bodily as well as ever," adds: "So far the treatment by ice with a view to procure sleep was a complete success, proving the ice application recommended by Chapman to be an invaluable therapeutic agent. . . . The value of cold to the spine cannot be over-estimated."

*Abstract published in the "British Medical Journal," December 12, 1868, of a Paper entitled "The Therapeutic Application of Heat and Cold."*

*Read at the Medical Society of the College of Physicians of Ireland, November 25, 1868, by Dr. D. B. HEWITT, L.K.Q.C.P., Physician to the City of Dublin Hospital.*

"After a short preliminary historical notice of the effects attributed to heat and cold as therapeutic agents, Dr. Hewitt proceeded to say that his chief object in bringing the matter under the notice of the Society, was to elicit the experience of any member who might have tried the method recommended by Dr. John Chapman of London. He did not think that a sufficient number of cases had as yet been adduced to furnish matter for an induction so extensive as to be probably true; but the facts as observed by him were quite in accordance with those detailed by Dr. Chapman. He had not seen any thermometrical observations respecting the effect of heat and cold to the spine, with the exception of those which he had brought under the notice of the Society last year. He then detailed several cases in which the use of those agents had been remarkably successful; and a few cases in which they fail to do good. Among the former, he enumerated a case of amenorrhœa, with very violent cephalalgia and spinal pain, in which, after the failure of drugs and dietetic treatment, the ice-bag had been used; the result being that

the headache and spinal pain were quite removed, and the menses returned, after the use of a blister to the sacrum, so profusely as to be almost menorrhagic. Ice had been used with excellent effect in two other cases of cephalalgia, but in these cases applied to the head. He referred to three cases of delirium tremens treated by Mr. Hamilton of Steevens's Hospital, by the ice-bag, as recommended by him last year; and he gave some interesting thermometrical observations of a case of delirium tremens lately treated by himself on the same plan. When the ice was placed along the spine, the temperature of the axilla rose, and the pulse was increased in frequency, while the profuse sweating was checked; afterwards it was thought well to put the ice to the head, when the result was diminished frequency of pulse and a fall in the temperature. A case of obstinate vomiting, occurring during pregnancy, which had resisted all treatment, was quite checked, and even nausea was removed by the application of ice to the lower dorsal and upper lumbar spines. In a case of spinal myelitis, the temperature was raised, the pulse diminished in frequency, while it became fuller and stronger, and hyperæsthesia of the upper extremities was removed by the use of ice, and the paralysis was greatly diminished. In a case of chronic hemiplegia, it had failed to relieve the rigidity in the flexor muscles of the forearm, or to benefit the paralysis; and in a case of chorea, the result was not encouraging. He referred to a case of chorea, published by Dr. J. H. Benson, in which this method was successful, after the use of a brisk purgative. In conclusion, Dr. Hewitt gave his experience of the use of the hot-water bag in some cases of bronchitis. One of these was a case of chronic catarrh; and, though no expectorant was given, in three or four days the expectoration had become much more abundant, the dyspnoea was greatly relieved and the cough considerably lessened. The thermometer revealed, on one occasion, after two hours' application of the hot water, a decrease of seven-tenths of a degree; and on another day the temperature decreased at first, but subsequently rose.

"Dr. Benson, jun., referred to several cases recently treated by him with the ice-bag. He believed the manner of its application, and the length of time necessary for its use, had not been sufficiently understood; and hence its frequent failures as a remedial agent. Dr. Atthill strongly advocated its use, and related a case of menorrhagia, in a girl aged fifteen, in which he had applied it with remarkable success. Dr. Belcher approved of Dr. Hewitt's paper, and noticed the treatise of Esmarch, 'On the Use of Cold in Surgical Practice.' Mr. Croly gave an interesting account of a case successfully treated by him with the ice-bag. Mr. Thorpe Porter had cured a case of hæmorrhage with it."

*From a "Traité Théorique et Pratique d'Hydrothérapie," par Dr. BENI-BARDE, Paris, 1874.*

"Whatever theory we may adopt, the therapeutical effects of the spinal ice-bag in the treatment of cerebral anæmia are evident, and we can affirm that, in a great number of cases, they are very salutary" (p. 793).

Referring to the treatment of vomiting, this author writes:—"Of all the means employed, Dr. Chapman's spinal ice-bag has appeared to us the most convenient, and certainly the most efficacious. . . . This method is inoffensive, easily practised, and, in the majority of cases, extremely useful. We know cases in which its therapeutical action is really marvellous. . . . We can affirm that the ice-bag has rendered us real service in several difficult and alarming cases" (p. 897).

Adverting to amenorrhœa, Dr. Beni-Barde remarks:—"The ice-bag so acts on the blood-vessels by means of the vaso-motor nerves as to force them to dilate, and consequently to render the circulation completely free. This paralytic effect extends to the vascular nerves of the feet, and manifests itself by the generation of heat in the extremities. We know amenorrhœic women having an intolerable coldness of the feet even in summer, who, under the influence of the lumbar ice-bag, have experienced at one and the same time the re-establishment of the menstrual function and the function of calorification" (p. 995).

Respecting menorrhagia, he says:—"We can affirm that Dr. Chapman's spinal warm-water bag renders real service in cases of hæmorrhage" (p. 1003).

*From Dr. GUÉNEAU DE MUSSY, Membre de l'Académie de Médecine, Médecin de l'Hôtel Dieu.*

This distinguished physician published in *Les Annales de Gynécologie* for July 1875 some remarkable cases of metrorrhagia arrested by the application of heat to the lumbar region. One of these cases was that of a woman who suffered very severely during six months from that disorder. She was for some months in the Hospital St. Antoine under the care of Dr. Brouardel, who "made use of the most varied remedies, comprising warm

baths, astringent injections, cauterizations of the womb, without effecting the arrest of the hæmorrhage. She then became discouraged, and obtained admission to the Hôtel Dieu. . . . She continued to lose blood copiously, and several times a day she expelled large clots. Examination of the womb revealed nothing capable of explaining the cause of the metrorrhagia. . . . I had recourse to the series of remedies which had already been adopted by M. Brouardel, after having tried in vain sulphate of quinine, and blisters to the hypogastrium. . . . I introduced solid nitrate of silver into the cavity of the neck of the womb, leaving it to dissolve there." All the means used having proved inefficacious, "I then decided to employ the remedy advocated by Dr. Chapman, and by means of his india-rubber bag," heat was applied to the lumbar region. "The applications were renewed every third hour. The next day, February 16th, the hæmorrhage had considerably lessened; the 17th, it was completely stopped." At the end of a month the menses recurred, and "soon assumed the appearance of a veritable hæmorrhage. . . . The patient swooned several times. . . . I applied the hot-water bag, and in thirty-six hours, as on the previous occasion, the hæmorrhage was completely stopped."

Dr. Gueneau de Mussy relates another case of a woman who was admitted under his care into the Hôtel Dieu on account of a severe metrorrhagia, probably due to abortion: "On the 25th of February warm water was first applied to the lumbar region by means of Dr. Chapman's water-bag. On the 26th the flow had diminished in a sensible degree, and on the 27th it had completely ceased."

Dr. de Mussy also mentions a case of suffocating catarrh complicating pulmonary tuberculosis, in which there were sub-crepitant rales throughout the whole chest with extreme dyspnoea, and he states that the patient "was quite relieved of this complication in twenty-four hours after the application of Dr. Chapman's hot-water bag."

In concluding his paper, Dr. de Mussy observes:—"The testimony of Dr. Atthill of Dublin confirms the assertion of Dr. Chapman, and authorises the conclusion that the subject in question is not merely one of those illusions that are too often encountered in the case of inventors of a therapeutic method."

*From the "Athenæum," August 15, 1868.*

"It is due, we think, to Dr. Chapman to say that he has been the first to make anything like a systematic attempt at applying our knowledge of nervous physiology to the treatment of diseases. . . . A little thought will show any one how comprehensive a theory of disease is stated in Dr. Chapman's nine propositions, and how universal a remedy is proposed. It is not alone diseases of the stomach and bowels to which heat and cold along the spine may be applied, but to diseases of the whole series of organs that are linked, as it were, to the brain and spinal cord by the action of the sympathetic nervous system. . . . We think his theories demand inquiry, and his practice a fair trial."

*Preliminary Objection to the Application of Ice along the Spine.*

"A great preliminary objection to Dr. Chapman's therapeutical method consists in the reluctance which patients, when they hear of it for the first time, very naturally feel to be treated by the application of ice along the spine. In order to overcome this feeling by showing its groundlessness, Dr. Chapman cites the testimony of a considerable number of patients who have been treated by ice in cases of sickness, diarrhoea, neuralgia, and various other disorders, and certainly there can be no mistaking the language of the patients themselves written down from their own lips. The following expressions are samples of the kind of testimony given by a large number of patients:—"The ice is beautiful; I don't think I shall ever be able to do without it—it is so comforting." 'I find the ice very agreeable; I look for it, and would like to have it on longer each time.' 'I go to sleep with the ice on; it's astonishing how pleasant it is.' In a report of a case of diarrhoea treated successfully by ice to the spine, the director of an hydropathic establishment remarks: 'One thing has much struck me, viz., the liking that sensitive chilly patients have for the cold bag to the spine, although frightened to think of it before they make a trial;' and Dr. Druitt, who, it appears, examined several patients treated by Dr. Chapman, says: 'I learned from all the patients that the treatment had made them more comfortable—I mean as regards their general feelings of health and animal sensations, without reference to the relief of particular symptoms.' We may add that in forty out of one hundred cases given in this volume\* the records of them prove that instead of inducing a feeling of coldness, the use of the spinal ice-bag so improved the circulation as to cause patients who had habitually suffered from that feeling to become permanently warm."—"The Doctor," April 1, 1873.

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### CONTENTS.

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III. The Pathology of Neuralgia and its Complications.	XII. Diagnosis and Prognosis.
IV. The Pathology of Visceral Neuralgia.	XIII. The Treatment of Neuralgia.
V. The Several Kinds of Superficial Neuralgia.	XIV. The Soothing and Agreeable Effects of the Spinal Ice-bag.
VI. The Several Kinds of Visceral Neuralgia.	XV. Neuro-Dynamic Medicine Exemplified: An Analytical Exposition of its Effects.
VII. Painful Excitability of the Spinal Cord.	XVI. Neuro-Dynamic Medicine Exemplified: A Series of Cases Illustrating its Principles and Practice.
VIII. Dr. C. B. Radcliffe's Theory of the Genesis of Pain.	
IX. The Predisposing Causes of Neuralgia.	

*From the "Medical Times and Gazette," June 14, 1873.*

"In the present volume, Dr. Chapman aims at something far more than a clinical generalisation. . . . He would tell us of the immediate agency by which each and all of these disorders (of the nervous system) is caused, and knowing this he would give us a key to the direct treatment of them all. . . . The author himself sees and welcomes the far-reaching generalisations which his method opens out. He does not shrink from saying that here we have a key to disorderly actions of muscles, voluntary and involuntary, to morbid actions of glands, to disorders of local nutrition, &c.; that his method is a method not only of interpreting and curing neuralgia, but of commanding a vast number of very various maladies. Indeed, although no vital function depends primarily and in its simplest terms upon the nervous system, yet in man this system has gained so great a control over all actions in the economy that to command this system is practically to command the whole body. Dr. Chapman's system is, therefore, something like a panacea, and he asks to have it regarded in that light, and to have it called the doctrine of 'neuro-dynamic medicine.' He chooses at present to work this out on the ground of neuralgia, but he does not conceal that his ambition has a far higher flight in contemplation. . . .

"Dr. Chapman himself, however, speaks not as a mere theorist. He publishes in adequate detail a very large number of important cases, which together make up a large mass of evidence in favour of his views—evidence which cannot be disregarded. . . . These numerous examples of positive success cannot be without great weight; many of them, indeed, are very striking. We do not shrink from saying that these cases are really very striking, for Dr. Chapman relates them in cautious and temperate language; indeed, his whole book is very ably and systematically written, and reads like a trust-

worthy treatise. . . . Granting Dr. Chapman's therapeutical results to be good, there are several important stages in his superincumbent arguments which as yet are far from secure, though we are unable, of course, to deny their possible validity. In conclusion, we consider that Dr. Chapman has written a very able book, based on observations and arguments which have evidently cost him much labour and time. We consider that in this book he makes a very strong claim upon the attention of his professional brethren, who are now bound to prove or disprove his allegations : we have, therefore, given the subject the space and the serious attention in our columns which so large and temperate a work fairly demands. We cannot now do more ; but it is evident that if Dr. Chapman establishes any great part of his thesis, he has made one of the most remarkable therapeutic discoveries in the history of the art."

*From the "Medical Press and Circular," April 30, 1873.*

"The work before us, from the pen of Dr. Chapman, presents an elaborate analysis of the symptoms of the whole group of neuralgic diseases, and an interpretation of those symptoms according to the theories of neuro-pathology to which the author has been led by observation of the effects of the particular system of treatment in connection with which his name is chiefly known to the Medical profession. The careful study of the natural history of the disease, and of the complications which it presents, evinces both careful and continued observation, and a thorough acquaintance with the literature of the subject. The facts and opinions of every writer on neuralgia, English and Foreign, past and contemporaneous, are laid before the reader fairly but critically, and in a manner which renders the work a useful contribution to medical literature, quite apart from the temets, the presentation of which is its special object, and which we consider it the duty of the profession to put to the test.

"It is, indeed, quite time that the remarkable generalisation long since arrived at by Dr. Chapman, and now supported by the vast array of facts and arguments contained in the volume before us, should receive the thoughtful consideration of all those engaged in the treatment of disease. His theory not only explains many morbid phenomena, but suggests a rational mode of treating the diseases on which they depend. This mode has been applied in hundreds of cases with such success that Dr. Chapman is entitled to ask his brethren either to try it in their own practice, or to state the reasons for not doing so, and we venture to assert that those who carefully read this volume will hesitate before they measure swords with the author. The work is an elaborate and well-sustained argument for the truth of the author's theory, and constitutes so important a contribution to rational therapeutics, that we proceed to furnish our readers with a detailed account of its contents."

*From "The Doctor," April 1, 1873.*

"This work, though an exhaustive treatise on neuralgia and kindred diseases of the nervous system, puts forth claims which, if conceded, would involve nothing less than a revolution in both pathology and therapeutics. A critical sketch of 'Pathological Theories of Neuralgia' is given in chapter ii. ; in chapter viii. Dr. C. B. Radcliffe's 'Theory of the Genesis of Pain' is subjected to a searching critical analysis ; and in chapter xi. Dr. Anstie's 'Pathology and Etiology of Neuralgia' is also elaborately summarised, while every argument in support of it is submitted to merciless scrutiny. The significance and bearing of every pathological fact adduced by these physicians in support of their respective, and, indeed, kindred theories, are carefully examined, and after weighing the whole of the evidence presented in favour of these theories, our author sums up its value in each case in a concluding paragraph, and gives judgment against them. As examples of critical analysis, logical acumen, complete mastery of the subjects dealt with, and crushing confutation of the doctrines discussed, the chapters deserve special attention.

"The wide range of subjects strictly cognate, however, and having a direct bearing on the main theme, which are discussed in this chapter [On the Pathology of Neuralgia], and the thoroughly exhaustive and scientific way in which they are handled, present striking evidence of the author's extensive knowledge, profound as well as logically consistent thought, complete familiarity with every aspect of the problem dealt with, and of his power of ab- all the facts and arguments relating to it, so as to converge the to lead his readers irresistibly to the conclusion, which it is to ablish. A glance at the topics discussed under the head of will suffice to give an idea of the

extent of area traversed, though, of course, it can convey no notion of the thoroughness and originality with which the work is done.

"We have only to add that the work, as a whole, reflects credit on the medical literature of the day. In a lucid style, and with logical precision, Dr. Chapman has expounded a theory that every practitioner ought carefully to weigh, and has enforced his doctrines with a mass of evidence that entitles him to a foremost place among those accurate clinical observers who are helping to advance scientific therapeutics."

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*From the "Lancet," February 20, 1864.*

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*From the "Medical Times and Gazette," January 23, 1864.*

"Dr. Chapman expresses his belief that many maladies not usually held to depend on nervous agency—such as those in the title (diabetes and constipation) really originate in that manner. . . The subject deserves the serious attention of the profession, not only from the high character of its originator, but also from the amount of evidence which he has already brought to bear upon it. He, however, seems to be as yet only at the outset of his investigations, and promises more detailed information hereafter. In so doing he will confer a great boon on physiology and practical medicine."

*From "Clinical Notes on Uterine Surgery," by J. MARION SIMS, M.D., p. 41.*

"Nor should he (the student) fail to study the brief monograph of Dr. Chapman on Cold and Heat in the Treatment of Functional Diseases of Women."

*From the "Medical Mirror," April 1867.*

"In his pamphlet, 'Functional Diseases of Women,' Dr. Chapman gives thirty-two cases which he had treated successfully either by cold or heat, or by both together, applied along the spine. Six of these cases were cases of defective and painful menstruation, which were cured by the application of the spinal ice-bag. . . One of the foregoing cases exemplified the curative power of the spinal ice-bag over leucorrhœa as well as deficient menstruation.

"In a very interesting section, entitled 'Coldness of the Feet; its Relation to Functional Diseases of the Womb, and its Cure by means of Ice,' he gives a number of cases proving that its application along the lower third of the spine causes the lower extremities of patients who had suffered habitually from coldness of the feet to become permanently warm. In our last number we published reports of two cases illustrative of the power of the spinal hot-water bag in arresting menorrhagia. We need, therefore, only allude to the six cases of this malady treated successfully by means of heat, and reported in detail in Dr. Chapman's pamphlet. He also gives a very curious case tending to prove that the development of the mammæ may be influenced by cold and heat applied to the back.

"In the 'Journal of Mental Science' for April, 1865, Dr. Chapman gives a remarkable case of the sickness of pregnancy which he had effectually controlled in the same way. The lady in question had had several miscarriages brought on by the violent retching incident to her sickness, which was always continued until the ovum was expelled; but, owing to careful treatment, by means of the spinal ice-bag, she was enabled when last pregnant to retain her food, and to carry the child to the full term.

"There can be no doubt that any man who will, with an unbiassed and philosophical mind, consider Dr. Chapman's cases, must admit that he has made a discovery which

promises to do much good to suffering humanity. . . . Not only is the result of ice effective towards increasing the vitality of the uterus, and promoting the period, but the pain is diminished. The dysmenorrhœa is, in fact, cured. . . . It would be well for the patients who are supposed to be suffering from mechanical dysmenorrhœa, if before the uterus was cut with hysterotomes, a proceeding in the hands of some practitioners not always free from danger, that Dr. Chapman's plan was made the preliminary treatment. Drs. Marion Sims, Greenhalgh, and Routh, would probably cut less often. . . . Section III. is the first philosophical essay that we seem to have read on cold feet. . . . We must with him admit that ice applied to certain parts of the spinal cord will promote the menstrual flow, and produce warm feet; and the nine cases mentioned are fully confirmatory of this view. . . . It cannot be doubted but that the obstetric physician may derive from these views of Dr. Chapman's great assistance in the treatment of many uterine cases, hitherto almost incurable, always tardy and tedious, both to patient and himself. Section IV. treats of hæmorrhage and menorrhagic pain cured by means of heat. We are almost better pleased with Dr. Chapman's results here than with the former. The correctness of former results following the use of ice, is greatly enhanced when we find exactly the opposite effects following the use of heat applied to the spinal centre. We cannot fail to be struck with the rapidity with which the menorrhagia is controlled. Ergot has not the same power; and the advantages of this plan in many cases of fibroids, change of life, flooding, is very conspicuous. We have read Dr. Chapman's book with pleasure, and we are pleased with his manner of working. It is clear he has made a very useful discovery and more useful applications, and he knows it. He is not an enthusiast simply. He is a devoted learner and a modest teacher."

From DR. ROUTH'S *Speech at the London Medical Society, March 18, 1867, reported in "Medical Press and Circular," April 3, 1867.*

"He said the method had been tried in a case of what he called 'convulsive action of the stomach,' associated with pregnancy. The patient suffered from sickness continually, for nearly three months; it was no use giving her drugs, for they came up again directly. She had been supported to some extent by injections per anum, but was in such a state of exhaustion that the question of inducing abortion, in order to stop the sickness, was entertained. By Dr. Chapman's advice, the spinal ice-bag was applied. The immediate effect was the production of refreshing sleep; by continuance of the treatment, the sickness steadily and completely subsided, and the patient is now well and gaining flesh. Dr. Routh said he had tried the method in a case of profuse menorrhagia; after the double-column hot-water bag had been applied during an hour, the flow ceased."

From DR. FULLER'S *Speech at the Harveian Society, November 21, 1867, reported in "Medical Press and Circular," December 11, 1867.*

"Dr. Fuller avowed himself a believer in spine-bags, the efficacy of which, he said, he had over and over again experienced in certain cases of menorrhagia and dysmenorrhœa."

In the *Introduction to his book on "Sea-Sickness, and How to Prevent It," second edition, Dr. Chapman refers to the following Medical Practitioners, who have informed him of their verification of the truth of his statements concerning the effects of the Spine-bags on the Uterine Circulation:—*

Dr. Brereton, of Sydney; Dr. de Faye, of Brussels; Dr. J. H. Benson, of Dublin; Professor Maclean, of Netley Hospital; Dr. Hayle, of Rochdale; Dr. Rayner, of Malvern; Dr. Barber, of Ulverston; Mr. P. Harper, Mr. Hammerton, Dr. Fuller, Dr. Gooldeen, of London; Doctress Denamore, of New York; and Miss Firth, lately Matron of the Endell Street Maternity Hospital, London.



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*From the "Lancet," March 4, 1865 (referring to the first edition).*

"Certainly, so far as the history of these voyages across the Channel goes, it is highly in favour of the author's ingenious recommendations. . . . We advise, both for practical and theoretical purposes, that the pages of this pamphlet be carefully perused."

*From the "Medical Press and Circular," 1867.*

"The reader will, of course, have perceived that the remedy to which we allude is that proposed by Dr. Chapman. The application of this theory to the treatment of sea-sickness, involves a consideration of the immediate cause of that distressing malady. Dr. Chapman is convinced that the proximate cause of sea-sickness consists in an undue increase of blood in the nervous centres along the back, and especially in those segments of the spinal cord related to the stomach and the muscles concerned in vomiting. Dr. Chapman alleges that the pallor and coldness of the surface characteristic of sea-sickness are due to the contraction of the peripheral blood-vessels in the way we have just explained, that the cold sweat, and copious secretion of mucus often expelled from the stomach, are due to excessive stimulus from the spinal cord, which he affirms to be the efficient cause of glandular action, and that, as is well known, the spasms of the voluntary muscles, proceeding in rare instances to convulsions, are also due to hyperæmia of the cord. This hyperæmia of the nervous centres is caused, as he maintains, by the motion of the vessel acting on the brain, the abdominal viscera, and the spinal cord; and if the theory be correct, it is obvious that, by preventing the hyperæmia, we take away a segment of the circle which must be complete in order to produce the disease. This he does effectually, if we may trust the reports of cases, by the application of ice along the spine. . . . According to the reports of patients, it not only arrests or prevents the sickness, but the cramps or spasms that frequently accompany it, at the same time restoring the circulation to its normal standard, and so changing the pallid, cold, prostrate condition of the victims of sea-sickness to the ruddy, warm glow characteristic of health and activity of the circulatory function. . . . It would seem from numerous instances that, properly applied, the ice is not only safe, but positively pleasant. . . . Not only is the ice not painful, but, according to the testimony of the great majority of patients, quite the reverse. . . . Moreover, the soothing effect is so general that sound and refreshing sleep is frequently induced, so that we constantly read of patients—men, women, or little children—falling asleep on the ice-bag and waking up refreshed and hungry."

*From the "Medical Press and Circular," November 18, 1868.*

"Whether Dr. Chapman's speculations as to neuropathy be true or no, there is no doubt that they contain many of the features of a correctly-formed theory. He gives us, in an admirably clear introduction, a synopsis of his views as to neuro-physiology, setting

out from the assumption that the sympathetic is the excito-motor nerve governing the vascular system, and that the functional activity of the glands is excited or maintained by a stimulus from the cerebro-spinal axis. The former of these assumptions is now pretty generally admitted since the clear demonstration given of the fact by Bernard and Séguard. The latter proposition is an extension of the views of Bernard, Ludwig, and Pfleger. Bernard proved that the parotid and submaxillary glands receive their nervous supply on the one hand from the brain and spinal cord, and, on the other, from the sympathetic; and demonstrated by experiments on animals that when the former are in action the maximum of blood is supplied to the glands and the maximum of saliva secreted, whilst, when the latter are in action, it modifies the volume of the arteries and regulates the supply of blood. Dr. Chapman states that he has discovered that, by applying heat along the spine, he stimulates the glands of the skin and mucous membrane, and by applying cold he restrains or depresses them, thus increasing or arresting their secretions. According to our author, then, the mucous and cutaneous glands act under the control of a special set of nerves derived from the brain and spinal cord, and distinct from the sympathetic. Glandular action, in short, and glandular inaction, are due, the first to a preponderance of cerebro-spinal influence, the second to a preponderance of sympathetic nerve force. He maintains that all glands possess positive motor nerve fibres from the cerebro-spinal axis, even in the cases where anatomy has not discovered such to exist. From these postulates, Dr. Chapman requires our assent to the importance of endeavouring in all cases of excessive discharges from glands, such as diarrhoea, leucorrhoea, bronchorrhoea, &c., to paralyse this cerebro-spinal influence, and thus to inhibit the supply of blood to the glands which causes the discharge.

"So much, in brief, for the theory; and now for the verification of the law. Our author points to the evidence adduced by a number of medical men, among whom we perceive with pleasure many most distinguished Dublin practitioners, as to the rapidity with which cases of diarrhoea, dysentery, delirium tremens, constipation, vomiting, dysmenorrhoea, amenorrhoea, menorrhagia and leucorrhoea, cholera and diabetes, have been cured. Paralysis and epilepsy have, according to much excellent medical evidence, been frequently cured by the application of this theory. If vomiting on land be under the dominion of this mode of treatment, we shall, of course, not be surprised to find that sea-sickness is amenable to it.

"The moral we would derive from perusing this admirably-written work is, that we ourselves feel emboldened to absolve ourselves from a rash vow never to cross the Atlantic. Armed with one of our author's ice-bags, we are determined to dare '*rabiem noti*;' and we invite all our readers to peruse this work carefully and without prejudice, and our medical ones to make a careful trial of a process which so rationally promises to abolish one of man's greatest sufferings, sea-sickness."

*From a Letter of S. M. BRADLEY, Surgeon, Cunard Service, in the "Lancet,"*  
December 3, 1864.

"In severe cases where other remedies have failed, I have very generally found it (the spinal ice-bag) do great good. I have applied it to young children, delicate women, and old people. In no case does it do harm; but in the great majority of instances it soothes the nervous irritability which so commonly accompanies sea-sickness, induces sleep, and so enables the stomach to receive light food, and consequently relieves exhaustion. . . . I order it to be kept on a couple of hours; though if the patient sleeps, as is often the case, I never remove it until after waking."

*From a Letter addressed to Dr. Chapman, June 3, 1865, by Dr. HAYLE of Rochdale.*

"I recommended a patient about to cross the Atlantic to try one of your ice-bags for sea-sickness. The result was most satisfactory. He was never sick when wearing the ice-bag. Once he went without it, and then, and then only, was he sick."

*From a Report of "Case of Sea-Sickness successfully treated by Ice to the Spine." By*  
B. LEE, M.D., *in the "Philadelphia Medical and Surgical Reporter."*

"The case (in question) is as conclusive as a single case can be, in regard to the great practical value of Dr. Chapman's discovery. . . . The effects of the application of the ice-bag were little short of miraculous."

The section of Dr. Chapman's pamphlet entitled "Cases and Results," contains reports from seven medical men, besides those supplied by Dr. Chapman himself, of cases in which the efficacy of the spinal ice-bag as a remedy for sea-sickness had

been clearly demonstrated. The first five of those reports are numbered respectively xviii., xxiv., xxv., xxxiii., xxxiv.; the remaining two, from surgeons of ocean-steamers, are given at page 110.

The following letter also records the experience of a physician :—

*"Quinta Andrado, Funchal, Madeira, Oct. 26, 1868.*

"Dear Sir,—Allow me to apologise that the exigencies of packing, leave-taking, and winding-up my affairs prevented my acknowledging your kind present of the spinal ice-bags. I took a brass basin and a blanket with some ice from Liverpool. The sea was very calm, and I was not sick for three days. My wife was sick the first day, and I applied the spinal ice-bag. She experienced considerable relief, but did not feel inclined to come on deck with the bag on her back, and soon fell asleep. In three hours she sent the bag to be refilled. She was able to eat some dinner, and felt the sensation of cold to the back agreeable. As my ice was now done, I did not again apply it.

"On the fourth day the ship commenced to pitch, and this made me sick, though I had become accustomed to the rolling. I applied for ice to the steward, but as the quantity on board was small and much needed for the African voyage, it was with some difficulty that I got enough partially to fill a bag long enough for me. The ice, too, melted very rapidly, and I had to break it in large pieces; it was not, therefore, applied to every portion of the spinal tract until a portion of the ice melted. For about half-an-hour I noticed no difference in the feeling of nausea. I then began to feel the desire to sleep, but resisted it, as I wished to note my sensations. It soon, however, overpowered me. I lay down on my cloak on deck and slept for an hour. I awoke quite well, and did justice to my dinner, but I was rather disappointed to note that the pitching of the ship was considerably abated.

"From my small experience, I should be disposed to recommend your bags in severe cases of sickness, on long voyages. I should expect them, where they did not cause complete recovery, to afford marked relief, induce sleep, and cause food to be borne. On short voyages, they must be very useful and save much suffering.

"I remain, Sir, yours faithfully,

"John Chapman, Esq., M.D., London."

"W. W. IRELAND, M.D.

## DIARRHŒA AND CHOLERA;

*Their Nature, Origin, and Treatment through the Agency of the Nervous System.* By JOHN CHAPMAN, M.D. Second Edition, enlarged, 8vo, cloth, 7s. 6d.

*From the "Medical Times and Gazette," November 3, 1866.*

"This is a remarkable book, and worthy the serious attention of every one of our readers who has the leisure and inclination to think out his own opinions upon an interesting and difficult subject. We do not say that the theory of cholera which the author proposes is correct, or that the treatment based upon it is sound. Such an admission is not necessary to justify our recommendation. Even a false theory when rightly constructed has its uses, and, instead of hindering, hastens the advance of knowledge. Every one possessing the slightest acquaintance with the history of astronomy knows that the doctrines of cycles, epicycles, and ellipses were begotten naturally and necessarily out of each other, and that if Kepler had not so often propounded speculative errors, Newton would not so often have hit upon speculative truth. When men of science disclaim hypotheses, they are either unfit for their vocation, or, like Newton, they are better than their creed. Hypotheses are at once the effects and causes of progress: and one might as well attempt to preserve and employ an army without organization, as to preserve and employ phenomena without a theory to weld them into one. But the theory must be provisionally, if not positively, true; it must be intelligible and consistent; it must explain a greater number of facts, and reconcile a greater variety of apparent contradictions than any which has preceded it, and it must have become developed not by the addition merely, but the addition and solution of subsidiary explanations.

"Now the book before us contains a speculation which, whether true or false, exhibits in its outlines, if not in its details, the essential features of a well-constructed theory.

It is in harmony with the results of the most recent physiological investigations: it is ingenious, clearly put, happily illustrated, logically argued, and meant to be a more comprehensive and simple explanation than has yet been given of the phenomena of choleraic disease.

"It is only by a close examination of the detailed application of the hypothesis as a means of rendering intelligible the proximate cause of every special symptom that a comprehensive conception of the hypothesis becomes possible.

"Each symptom receives a consistent and intelligible explanation, but we would direct the reader's attention more particularly to the author's exposition of the immediate causes of the increase or persistence of heat in the body after death; of the rise of temperature in certain parts before dissolution; of post-mortem muscular contractions; and of the differences in the relative activity of the positive and negative motor nerves in different cases.

"We must pass over the chapter on the causes of cholera, which displays great originality and ingenuity in reconciling and explaining the various modes of action of causative agencies, and a singularly happy power possessed by the author of using his knowledge for the setting forth of new analogies, and the bringing together apparently the most contradictory phenomena for the support of a general law.

"If Dr. Chapman's theory of cholera should turn out to be correct, it would seem obvious that the only rational treatment consists in the adoption of such means as will most rapidly and completely subdue the hyperæmia of the spinal cord and sympathetic ganglia, and re-establish the healthy equilibrium of the circulation. Dr. Chapman thinks that the time may come when this will be effected by the proper application of galvanism; but he maintains that in the present state of knowledge there is 'no available power of subduing hyperæmia of the automatic nervous centres comparable to that of ice applied along the spine.' In proof of this, he appeals to the testimony recorded by himself and others, that the leading symptoms of cholera—vomiting, purging, coldness, and cramps—when met with separately as manifestations of other diseases, are capable of being subdued in the majority of cases by the application of ice along the whole or a part of the spine.

"Dr. Chapman's treatment of cholera consists essentially in the application of ice to the spine and of heat to the general surface; and he is emphatic in insisting that the one should not be used without the other. As importance is attached to the exact manner in which this method of treatment is tried, it is but fair that those intending to test the value of Dr. Chapman's therapeutical proposal should make themselves familiar with what he says before beginning their experiments.

"The work concludes with a record of cases and an analysis of results.

"Whatever may be the final judgment pronounced upon Dr. Chapman's theory of cholera, it must be admitted that he has said nearly all that could be said in its favour. Its strength lies in its comprehensive and simple explanation of seemingly contradictory phenomena by the application of a recognised general truth; its main, and in our eyes great, weakness lies in the denial of any primitive affection of the blood. But even if wholly false, the theory will play an important part in the discovery of that which is true. The work is well written, methodically arranged, connected in all its parts by a pervading unity of design, and will take a permanent place in the history of the disease of which it treats."

*From the "Medical Press and Circular," Nov. 6, 1867.*

"The part of Dr. Chapman's work devoted to Cholera comprises six chapters, entitled respectively—(1) Definition, History, and Symptoms; (2) Post-mortem Phenomena in Cases of Death during Choleraic Collapse; (3) Pathology of Cholera; (4) Causes of Cholera; (5) Treatment, comprising (a) treatment by cold and heat, and (b) treatment by medicines; (6) Cases and Results. Chapter I. we must pass over altogether. Chapter II. contains a concise yet detailed description of all the recognised phenomena observable after death during collapse; and every one of these, as well as the several symptoms of the disease characteristic of its successive stages, receives an elaborate and complete explanation in the course of Chapter III., which is distinguished alike by the originality of the views it expounds, the ingenuity and cogency of the arguments by which they are enforced, the faithfulness to well-established facts which by way of confirmation or explanation are referred to at every step, and by the mastery with which the author co-ordinates and uses his abundant materials for the establishment and maintenance of his doctrines. . . .

"Unable to dwell longer on the author's strikingly original exposition of what he calls the negative phenomena of cholera, we select for quotation a few paragraphs which throw a flood of light on certain facts which, thoroughly established, have hitherto remained as utterly inexplicable as they are astonishing. . . .

"Whatever doubts may be entertained concerning Dr. Chapman's etiology of cholera, there can be none as to the consummate skill with which the facts adverted to in each section of this chapter are ranged and presented so as to establish the doctrine which the author advocates. The various influences enunciated have been adverted to by many previous writers, but, so far as we are aware, the several links in the chain of causation by which these influences produce the phenomena of cholera, have never before been exhibited : it is in this respect that the chapter under consideration displays an amount of insight and originality, as well as logical cogency, which cannot fail to commend it to every philosophical mind. It is one thing to see that there is some causal connection between great atmospheric heat, wide ranges of temperature, prolonged marches, and influences operative during the night, on the one hand, and the origination of cholera on the other ; but it is quite another thing to show distinctly, and in detail, the *modus operandi* of these agencies ; this achievement, in respect to each agent discussed, constitutes the characteristic excellence of Dr. Chapman's elaborate exposition."

*From the "Medical Mirror," March 1867.*

"The light thrown by these original ideas on the phenomena in question during the successive stages of cholera must, we think, impress every reader with the conviction that the hypothesis propounded by Dr. Chapman, whether absolutely true or not, at least affords a complete solution of a pathological problem of the first magnitude, and fulfils all the requisites which a true explanation supplies. . . . It embodies a great amount of novel truth ; it is ingenious, well reasoned, admirably supported, and not only in harmony with, but in advance of, the results of the highest investigations of the time. The subject is treated with real perspicuity and candour, and with a remarkable desire to appreciate every fact at its true value ; and the work, as a whole, lacks nothing that is needed to make it a rare specimen of the application of the severest logic, and the most precise manipulation of language to practical science. Indeed, the book is characterised not only by great ability, by originality of thought, by judicial acumen, and by familiarity with the spirit and tendencies of modern research, but also by a rare power of reconciling apparently contradictory phenomena, and marshalling them together for the support of a common purpose."

*From the "Journal of Mental Science," Jan. 1867.*

"Dr. Chapman applies his well-known views of the pathology of disease, and of its treatment through the agency of the nervous system, with wonderful ingenuity to explain all the phenomena of cholera. . . . Apart from all peculiarities of theory on the author's part, the present work will be found to contain a clear and complete account of what is known of cholera, and an acute and instructive criticism of the theories of its nature, which have been propounded by different writers."

*From the "Popular Science Review," Edited by HENRY LAWSON, M.D., Jan. 1867.*

"Whatever amount of truth Dr. Chapman's hypothesis may possess, his view is worked out with a display of logical reasoning, formidable facts, and erudition, such as is seldom met with in medical essays. . . . Of the hundred and one treatises on cholera which have been published during the past year, Dr. Chapman's is at once the most interesting, the most scientific, and the most scholarly."

*From the "Indian Medical Gazette," Jan. 1867.*

"The section criticising Dr. George Johnson's castor-oil treatment and lung-capillary theory is very ingenious."

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**CASES OF DIARRHŒA AND CHOLERA :**

TREATED SUCCESSFULLY THROUGH THE AGENCY OF THE  
NERVOUS SYSTEM, CHIEFLY BY MEANS OF THE

**SPINAL ICE-BAG.**

8vo, 64 pp., price 1s.

*From the "Medical Times and Gazette," Feb. 17, 1872.*

" . . . The author having proposed his method of treatment, has in the next place to determine its actual value in practice ; he accordingly completes his case by bringing forward a large body of evidence to show that his method, when carried out by attendants with the minute care he has a right to enjoin upon them is, if not uniformly, at any rate very largely successful. . . . We must say that his statements are backed by very strong evidence, used with much knowledge and skill—so much so, indeed, that they cannot be overlooked, but claim our instant attention. . . . We cannot but be pleased to think that the evidence is most strong in support of that one position in which we are most immediately interested—namely, that the ice treatment is successful. This surely is the main point, and in the face of the miserable results obtained under the use of other methods, we cannot but form a favourable estimate of the alternative treatment here proposed. . . . And although we fortunately have no experience of cholera thus treated, yet in fairness to Dr. Chapman, we ought to say that we have found the ice-bag very useful in some other disorders. For example, we have found it strikingly successful in maniacal conditions, when applied to the cervical region ; in sympathetic vomiting likewise, and in some other conditions too numerous now to mention."

*From the "Medical Press and Circular."*

"Dr. Chapman has the undoubted merit of originating a system of treatment which we believe is of great practical value. We are satisfied of the truth of his assertion, that the condition of the viscera of the thorax and abdomen can be modified to a considerable extent by the external application of heat and cold to the spine. We have ourselves observed the advantages of Dr. Chapman's method of treatment in sea-sickness. In the stages of bronchitis, where the breathing is dry and tubular, Dr. Chapman's hot-water bag often causes marked relief, and the increased secretion of mucus from the bronchial tubes seems to be proved by the greater softness of the breathing which may be ascertained to follow in a few minutes by auscultation. . . . The pamphlet is in a readable form, and shows both great scientific knowledge and practical sagacity."

*From the "Edinburgh Medical Journal," July 1872.*

"A readable and persuasive pamphlet. We should like to see Dr. Chapman's views fully tested."

London : BAILLIÈRE, TINDALL, & COX, 20 King William Street, Strand.

**Directions for Selecting a Suitable Bag in each Case.**

When an **ice-bag** is wanted for any particular patient, one should be chosen which will extend from the second or third cervical to the third or fourth lumbar vertebra, or from the nape of the neck to the lower part of the "small" or hollow of the back. It is exceedingly important that the cold should not (unless in certain cases for special reasons) extend on to the base of the skull, and even more indispensable that it should not reach over any part of the sacrum.

If it be desirable to apply ice to a portion only, say a third, of the spine, it is especially necessary that a bag of suitable width be used. Ice applied in one of the small-sized bags to the back of an adult would not produce an adequate effect ; whereas, applied to the back of a child in one of the large-sized bags, it would probably, by its great lateral extension, exert a depressing and otherwise injurious influence.

When it is necessary, in cases of children and youths, to apply ice to a part

only of the spine, a bag of suitable size for the patient in question, supposing ice were about to be applied to the whole spine, should be selected ; then, if only a third of the spine, no matter which part of it, is to be acted on, the upper cell of the bag should be filled with ice and applied where intended, the remainder of the bag being allowed to hang loosely, or folded up behind the cell which is used ; if two-thirds of the spine is to be acted on, the upper two cells of the bag should be filled with ice and applied to the appropriate part, the lower cell remaining empty and dependent, or folded upwards behind the cells in use, in the same way as when only one cell is used.

In applying ice to a limited part of the adult spine, either a spine-bag of suitable size for an adult should be used in the manner just indicated, or a short bag, called the "Lumbar Ice-bag," consisting of only two cells, and designed especially for the application of ice to the lumbar region, may be conveniently adopted.

**The Spinal Water-bags**, excepting the one of the smallest size, are all of one width, and are all alike intended for adults. The length, therefore, of the bag, which it may be desirable to employ for the application of the heat (or of moderate degrees of cold) in any particular case, must, of course, be determined by the medical attendant of the patient.

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#### **Directions for Preparing and Applying the Spinal ice-bag.**

Put ice (broken into pieces about the size of a nut or small walnut\*) into the opening of the bag, on the side nearest to the loops, until the contents of the lowest cell reach up to the bottom of the second cell ; then put ice in the middle opening until it reaches up to the bottom of the third cell ; and, finally, fill the top cell. Then close the bag with the clamp, *placing it on the thickened band-like part around the mouth*,† and letting the screws be on the same side as the loops of the bag. When the clamp is properly applied, a very moderate pressure by its screws is needful to make the bag water-tight. All pressure beyond what is needful injures the bag. Care must be taken that each cell is not so filled as to cause it to become round, otherwise only a small portion of the bag will touch the back. It is also expedient that the contents of the several cells should only slightly overlap each other.

In applying the bag, place the smooth side of it in apposition with the back, the side bearing the loops being turned outwards. The bag may be kept in its position in various ways. If it is intended to be used for only a short time, or if the patient be in bed, the best plan is to lie upon it, taking care that it is exactly along the centre of the back. If the patient should wish to move about, pass the elastic band round the head, in order that it may hold the upper cell close to the back of the neck, and sustain the bag by fastening the clothes tightly outside of it ; or pass a long tape through the

\* This is most easily done by means of an "Ice-breaker," supplied by ironmongers, and consisting of a tapered piece of steel, sharp-pointed, and fixed in a handle. The ice, when about to be broken into small fragments, should be placed in a wooden bowl, which both prevents them from being scattered and the ice-breaker from being blunted.

† These words are printed in italics in the hopes of emphasizing the injunction they contain as strongly as possible. Many persons apply the clamp beneath the band, where, as there are two layers of india-rubber less than there are above, it is necessary to screw it more tightly than is needful when the clamp is applied on the band, in order to make the bag water-tight. By thus wrongly applying the clamp, its undue pressure is liable to cut the bag, and so destroy it.

lower loop, carrying each half of this tape over the shoulders, crossing one over the other in front of the chest, carrying them backwards round the waist, in order to clasp the bag closely into the small of the back, and then, bringing them forward, tie them in front. The bag will thus be sustained, and without allowing any of its weight to depend from the head. In this case the patient may move or walk about without any discomfort while wearing the bag.

As ice contains a considerable amount of air, this air, as the ice melts, accumulates at the top of the bag, and being a bad conductor of heat, prevents the still unmelted ice which it surrounds from exerting its intended influence: it is therefore necessary to unscrew the clamp occasionally, in order to let the air escape, and, if the bag is to be worn for a considerable time, to replace the air by a little fresh ice.

Ice may usually be procured of any fishmonger; but patients who may need to use the spinal ice-bag for a considerable time will find it best to buy a quarter or half cwt. of ice at a time. The best ice may be had at from 5s. to 8s. per cwt., and may be kept in a refrigerator or common box. A refrigerator or ice-safe not only excludes the outer air from the ice, and thus retards its melting, but it is at the same time a domestic luxury.

Ice can, however, be so kept as to retard its melting without the aid of a refrigerator. For this purpose it should be placed in a wooden box having holes in the bottom, and so supported as to permit the water to drain away from it, and the ice should be surrounded with an abundance of sawdust. Whatever plan is adopted, the more the box containing the ice is surrounded with some substance which is a bad conductor of heat—a blanket, carpet, or some straw, for example—the more slowly the ice will melt.

### **Directions for Applying the Spinal Water-Bag.**

Fill the bag with water of the required temperature, and apply it directly to the appropriate part of the back, keeping it in its place in any way most convenient to the patient. This may be most easily done by lying upon it, or by pressing the bag between the back of a high-backed chair and the appropriate part of the spine. When the bag is applied in the dorsal or lumbar region only, it may also be sustained by merely fastening the dress tightly over it. In applying the bag in the lumbar region, ladies most usually keep it, as well as the lumbar ice-bag, in its place in this way, and can thus freely walk about during the application.

The spinal water-bag is used for the application of heat, and also of cold of any degree above that of freezing-point, in cases where it is desirable, while influencing the sympathetic ganglia on each side of the spinal cord to affect the spinal cord itself as little as possible; but its chief use is for the application of heat.


For the latter purpose the temperature of the water put into the bag may vary, according to the requirements of the case in question, from 110° to 120° Fahrenheit. In the great majority of cases in which the application of heat is desirable, the water used should not be of a higher temperature than 120°; and the lower the temperature of the water used consistently with effecting the object for which it is applied, the better for the patient. When it is necessary to continue the application, the water should be changed about every 30 or 45 minutes. Of course, the more the bag is shielded from the surrounding air by materials which impede the passage of heat—as woollens, leather pillow—the longer it will keep hot.



# DR. CHAPMAN'S SPINE-BAGS (PATENT),

DESIGNED FOR THE PRACTICAL APPLICATION OF THE PRINCIPLES  
OF NEURO-DYNAMIC MEDICINE.

THE SPINAL ICE-BAGS vary in breadth from two to four inches and a quarter, the shortest bag being the narrowest, and the longest the broadest. Their lengths are as follows :—



8 inch	} suitable for children.
10 "	
12 "	
14 "	
16 "	} suitable for youths of both sexes.
18 "	
20 "	
22 "	} suitable for women.
24 "	
26 "	} suitable for men.

Lumbar Ice-Bag, 10 inches long.

THE SPINAL WATER-BAGS are, in respect to size, as follows :—

6 inch	} suitable for youths of both sexes.
8 "	
10 "	} suitable for adults of both sexes.
12 "	
14 "	

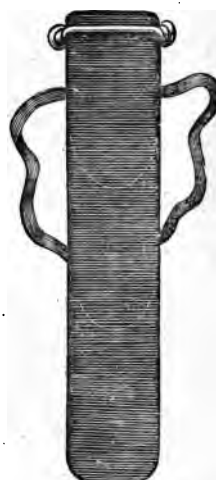
Lumbar Ice-Bag, 10 inches long.

THE SPINAL WATER-BAGS are, in respect to size, as follows :—

6 inch	{	suitable for youths of both sexes.
8 "		
10 "	{	suitable for adults of both sexes.
12 "		
14 "		



SPINAL ICE-BAG.



SPINAL WATER-BAG.

THE SPINAL ICE-BAG is divided into cells, generally three. By this arrangement the ice, being prevented from falling from the upper parts to the bottom of the bag, can be kept in apposition with the whole or any special part of the spine, even though the patient should be upright, or should walk about. The mouths of all the cells are so effectively closed by means of a clamp that no water can escape even though the whole of the ice be melted.

The Bags are sold by the following Agents :—

LONDON : C. MACINTOSH & CO., the Manufacturers, 83 Canon Street, E.C.

PARIS : ROBERTS & CO., 23 Place Vendome.

J. P. HOGG, 2 Rue Castiglione.

AND MAY BE HAD BY ORDER OF ALL DRUGGISTS.

**SEA-SICKNESS**  
**PREVENTED OR CURED**  
**BY MEANS OF**  
**DR. CHAPMAN'S SPINAL ICE-BAG:**

*Directions how to use it.*

Persons intending to make use of the Spinal Ice-Bag as a remedy for sea-sickness must carefully comply with Dr. Chapman's Instructions given in his work, entitled, "Sea-Sickness, and how to prevent it" \* [see page 11 of this pamphlet]. A summary of the most important parts of those instructions is here presented.

The Spinal Ice-Bag must in each case be of a size exactly suitable to the patient—in respect both to length and breadth. (See pages 16 and 17 of this pamphlet.) Each of the three compartments of the bag must have ice put into it in the manner directed at page 17. The bag must be applied along the whole length of the spine, extending from the upper part of the neck to the lower part of the hollow of the back. In all severe cases the bag should be placed next to the skin, but in many cases it suffices to place it outside the shirt or chemise. In no case must flannel intervene.

In order to apply the Spinal Ice-Bag next the skin, it suffices in the case of men to unbutton the waistcoat and shirt-collar and to loosen the braces, as well as the neckerchief, so as to permit the bag to be easily slipped down from above between the shirt and the spine to its proper place—where it is maintained without difficulty by the pressure of the clothes rebuttioned over it. The shirt-collar, unless unusually wide, cannot be rebuttioned, but the neckerchief, being retied, keeps it close, so that the fact that it is not buttoned is scarcely, or not at all, observable. Women about to apply the Spinal Ice-Bag must, of course, thoroughly loosen the stays as well as the other parts of the dress surrounding the chest, and when the bag is slipped down between the clothes and the spine, care must be taken that the bottom of the bag passes just between the waistbands sustaining the skirts and the lower part of the spine. The bag is then rightly placed, and is easily maintained in its position by tightening the clothes around it. The patient with the bag thus adjusted may sit up or walk about the deck without any discomfort. He, or she, will probably feel, however, that the agreeable sensation produced by the ice would be increased were the Spinal Ice-Bag pressed more thoroughly against the spine than it is by the dress even though it be felt to be quite tight. In such cases, if the patient will throw a rug or shawl across the shoulders, and will grasp it firmly with each hand, he or she may thus press the bag quite closely to the spine while walking about. The same object may be easily effected, if the patient sits down, by pressing the bag against the back by means of the back of a chair, or of any other support leaned against. In very severe cases the patient should lie on the Ice-Bag, care being taken that it presses as thoroughly against the lower as against the upper part of the spine.

So long as the liability to sickness continues, the Spinal Ice-Bag must be applied as uninterruptedly as possible. Each bag of ice, if the ice be good, lasts, when applied, about two hours (more or less according to the temperature of the atmosphere); and as soon as the ice in the bag is *wholly* melted, it should be refilled with ice, and should be at once reapplied in cases in which the tendency to sickness quickly recurs.

Persons crossing the British Channel between Dover and Calais, or between Folkestone and Boulogne, need, as a rule, only one bag of ice for the passage; and if, immediately before leaving London, Paris, or Brussels, to cross the Channel, they have the bag filled with good ice, and then carefully surrounded with several folds of a good woollen rug, and if they proceed directly from the railway to the boat, the ice in the bag will have melted very slightly meanwhile, and will still suffice to prevent sea-sickness during the passage.

Persons peculiarly liable to sea-sickness should always take care to have the Spinal Ice-Bag properly applied *before* the vessel in which they embark leaves the harbour. Some persons prudently apply the bag while they are still in the railway carriage, just before the train arrives at the port of embarkation.

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\* The work contains reports of about forty cases, proving that sea-sickness is both preventable and curable. As it is extremely desirable and important, on public grounds, to accumulate a large number of reports of the efficacy, or at least of the action and results, of the remedy in question, ~~persons making use of it are~~ <sup>persons making use of it are</sup> requested to send such reports, however brief (but of course the more circumstantial the better), of their experience of it to Dr. CHAPMAN, 212 Rue de Rivoli, Paris, who hopes, by incorporating them in the next edition of this work, to supply his readers with additional evidence of the value of his discovery, as well as of the facility and comfort of its application.





